Flexibility of the labor market

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To my parents and to Tania

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Chapter 1

Introduction

This dissertation is on the flexibility of the labor market. Although all the chapters were written with different motivations, there are links between them. This introduction summarizes the thesis while highlighting the links between the chapters. In section 1.1, after we elaborate on the common research theme, we develop the motivation of the thesis and the main research questions. In section 1.2 we summarize the research and the main conclusions.

1.1 Motivation and questions

The Great recession has represented the most severe deterioration of the labor market conditions since the late 1940s. Several labor market indicators such as, labor force participation, unemployment, employment, and hours have been affected. Labor force participation has declined both at the intensive but also at the extensive margin. We have been experiencing world wide increasing unemployment rates and unemployment durations and it is not clear where the solution lies. This dissertation analyzes several dimensions of the labor market in an attempt to understand the effect of government policy, the reasons why unemployment durations might go up and the consequences of job loss for earnings.

This dissertation is comprised of three essays that each deal with a different dimension of flexibility of the labor market. First a policy change: reduced working hours. Second, changes in unemployment dynamics: increased unemployment duration and third, job loss and its consequences for earnings of displaced workers.

In this dissertation the following questions are discussed:

- What are the effects of a working time reduction?
- Why did the median unemployment duration go up in the U.S. since the early 1990s?
- What are the determinants of earnings losses of displaced workers? Do occupational or firm specificities explain part of the earnings losses?

Chapter 2 addresses the first question. This chapter investigates the effects of a working time reduction, under full wage compensation, in Portugal from 44 to 40 hours. We find that workers involved in this change reduced their job separation rate and increased hourly wages, keeping monthly earnings approximately constant. The working hours reduction also affected workers working less than 40 hours per week; they were more likely to lose their job. Finally, we find evidence that the working hours reduction had a positive effect on employment through a fall in job destruction.

The second question is addressed in Chapter 3. Composition effects related to age played a significant role. However, apart from this rather mechanical impact, important structural changes, were at play. We have identified a major force reshaping the unemployment duration distribution: the change in sensitivity of the signal of schooling increased the median unemployment duration by 2.7 weeks. We argue that the signaling power of schooling during the recent low-unemployment environment faded significantly. When the unemployment rate is low, the information that is passed to the employer through the schooling signal does not promote more job offers to the more educated unemployed.

In Chapter 4 the last question is addressed. We find that the earnings losses are rather severe and persistent, representing around 50 percent of the pre-displacement wages, six years after the separation event. Those losses are explained by the joblessness experience of the displaced workers. We explore the sources of those losses, and we find that the allocation into lower-paid job titles accounts for half of the total average wage loss. Sorting into low wage firms also plays a significant role in explaining the wage loss of displaced workers.

1.2 Summary and conclusions

In the past decades, working hours have been reduced in many countries, with the idea that a reduction of working time per worker would increase the number of employed workers. Chapter 2 looks at the impact of a 1996 Portuguese law that reduced the maximum standard workweek. Our study is distinct from Varejão (2005) who investigates the effects at the establishment level. We investigate the effect of the working time reduction at the level of the individual worker allowing for an interaction between individual and firm effects. We analyze the impact of the reduction of the workweek assuming that the policy change resembles a natural experiment. The treatment group consists of all individuals who worked more than 40 hours in October 1996; the control group consists of workers who worked 35-40 hours in October 1996. The working hours categories are defined on the basis of the situation in October 1996, just before the introduction of the working time reduction. Assuming that the workers working less than 40 hours per week were not affected, the general calendar time effects are represented by the calendar year dummies, assuming that the differential development of the affected workers is represented by the working hours categories. When investigating the policy impact we know that the adaptation of hours comes mainly from the labor demand side and therefore, it is important to consider firm information. We consider that it might be that the treatment effects are influenced by the firm share of workers that worked more than 40 hours per week. After all, firms' costs of the working hours adjustment increase with the share of workers affected. The reduction of the maximum standard workweek from 44 to 40 directly affected about half of all workers in Portugal since they had a standard workweek of more than 40 h. Initially, the reduction of working hours was compensated by the use of overtime. Hourly wages of the affected workers increased, reducing their monthly earnings only slightly. Workers in the category 40-42h were less likely to separate from their firm. Due to spillover effects at the firm level the working hours reduction also affected workers working less than 40 hours per week.

The possible worksharing mechanism as a reaction to the reduction of the working time is also considered in Chapter 2. Employment effects of working hours reductions are not easy to establish empirically and indeed previous studies examine the impact of working hours reductions on the employment position of individuals but do not address overall employment effects. The main reason for the lack of evidence on the overall employment effects concerns the lack of information about the number of workers that find new jobs through the birth of firms.

We use a longitudinal Portuguese matched worker-firm dataset called Quadros de Pessoal (QP - "Lists of Personnel"). Reported data cover all the personnel working for the establishment in a reference week in October (see Cardoso (2006) for details). Every year QP gathers information for more than 250 thousand firms and 2.5 million workers. We use information for the time period 1994-1998. QP collects information on both standard and overtime hours in the reference month; a posteriori we transform them into weekly hours. Standard hours are defined as the hours worked during a normal week at the going wage. Overtime hours are the weekly hours worked at an overtime premium (50% for the first hour, 75% for additional hours). Monthly earnings are the monthly payments associated with the standard workweek. We use the national consumer price index to transform nominal earnings into real ones. The hourly wage is computed as the ratio of monthly earnings and standard hours of work.

At the individual level it is rather straightforward to use workers working above the "new" standard hours before the policy change as the treatment group and workers working at or below the "new" standard hours before the policy change as a control group. However, establishing the overall employment effects is a nontrivial exercise as there is no control group for firms that were created after the introduction of the policy change. In this paper we attempt to establish overall employment effects. There was considerable regional, sectoral and firm-size variation in the share of workers who were affected by the working hours reduction. If the reduction in working hours affected employment, it is likely to have had a bigger impact when the share of affected workers was high. Therefore, we can exploit the variation across labor markets to assess the impact of the workweek reduction. To do so we perform an analysis on the level of labor markets defined by industry, region and firm size. This aggregate approach allows us to study job creation and job destruction as well as worker accessions and separations and thus the net employment effects. Previous empirical studies suggest that reductions of standard working hours do not have positive employment effects and attribute this null effect due to adjustment of the hourly wage. However, previous studies only measure partial employment effects, while in our study we consider overall employment effects. We find evidence that under full wage compensation, the working hours reduction had a positive effect on employment through a fall in job destruction. We can only speculate about why reducing standard working hours in Portugal increased employment whereas in other countries no such effects occurred. Most likely, the increased flexibility in the use of the standard workweek made it easier to adjust the workforce at the intensive margin rather than at the extensive margin. To the extent that this reduced labor costs, job destruction was also reduced, causing positive employment effects. The reduced labor costs didn't stimulate job creation which may have to do with strict employment protection in Portugal.

Under full wage compensation, each hour becomes more expensive and the worker becomes cheaper in relation to hours. One could expect an increase in hiring, but given the difficulty to fire workers in Portugal, the firm will not fire, instead it will fire less workers. Therefore, the working hours reduction had a positive effect on employment through a fall in job destruction.

Since the early 1990s unemployment duration in the U.S. increased substantially relative to unemployment rates. In Chapter 3, we rely on censored quantile regression methods to analyze the changes in the U.S. unemployment duration distribution. Quantiles seem appropriate to analyze unemployment duration for two main reasons. First, the methodology estimates the whole quantile process of duration time conditional on the attributes of interest, which constitutes a complete characterization of the distribution of duration time. Quantiles provide a natural way of characterizing important concepts such as short- or long-term unemployment, by focusing on the relevant tails of the duration distribution. Second, from a methodological standpoint, it is worth observing that quantile regression provides a unified and flexible framework for such an analysis.

The data used in this inquiry is taken from the U.S. Displaced Worker Surveys of 1988 and 2008. The DWS is a retrospective survey that has been conducted biennially since 1984. It contains information on the nature of the job loss and the subsequent joblessness duration of workers displaced by reason of plant closure, slack work, or abolition of shift or position. The DWS is particularly well suited to study the distributional shape of unemployment duration because, unlike the CPS, it is a representative sample of the flow of displaced workers and because it provides information on completed spells of unemployment.

Changes over time in the distribution of unemployment duration may be framed as resulting from changes in the distribution of the conditioning variables such as the age distribution or from changes in the conditional distribution of duration itself. We use Machado and Mata (2005) method to disentangle those effects. The basic building block is the estimation of the conditional distribution by quantile regressions; then, by resorting to resampling procedures, one estimates marginal distributions consistent with the estimated conditional model as well as with hypothesized distributions for the covariates. Comparing the marginal distributions implied by alternative distributions for the covariates one is then able to perform counterfactual exercises that isolate the different effects contributing to the overall change.

Composition effects related to age (but not gender) played a significant role. But, apart from this rather mechanical impact, important structural changes, captured in the changes of the regression coefficients, were at play. We have identified a major force reshaping the unemployment duration distribution: the change in sensitivity to schooling increased the median unemployment duration 2.7 weeks. We argue that the signaling power of schooling during the recent low-unemployment environment faded significantly. When the unemployment rate is low, the information that is passed to the employer through the schooling signal does not promote more job offers to the more educated unemployed.

The determinants of earnings losses of displaced workers are addressed in Chapter 4. The first goal in this study is to investigate the monthly earnings losses by following Jacobson et al. (1993) (JLS) methodology, including transitions to zeros whenever the individuals are out of work. The second objective is to extend the Jacobson et al. (1993) (JLS) methodology by incorporating firm and job title fixed effects in the monthly wage equation (excluding transitions to zeros), allowing us to estimate the monthly wage losses of displaced workers. We decompose the monthly wage losses into their main sources using the methodology developed in Gelbach (2010).

Potential losses of displaced worker can be related to the firm and job-title that they held before and after displacement. The heterogeneity among firms' wage policies is very large and accounts for more than one third of the wage total variation (Torres et al. (2012)). Different wage policies are favored by the existence of industry rents (due to unionization or incentive pay premiums) or the operation of wage efficiency policies. In such an environment, the worker may benefit from engaging in job search to locate the firms with more suitable (more generous) wage offers. Good matches will be made and survive. Bad matches will be resisted and undone. However, with the occurrence of a displacement event, successful job searchers may loose their "job shopping" investment.

The role of job-title heterogeneity explaining total variation is also significant (around 50 percent). Job-titles summarize the general and specific skills of the worker, in particular those that are industry and occupation specific. Given the way those job titles were identified, they may also reflect the bargaining power of the workers. Because job-titles contain the skill requirements of the position held by the worker, it will also retain the hierarchical standing of the workers. Again, with the event of a displacement, a human capital will be destroyed, largely associated with the loss of his pre-displacement job-title. This was previously measured by looking at the effect of industry and occupation mobility. We now address directly this source of wage loss by looking at job-title fixed effects.

To properly incorporate these plethora of wage determinants a wage equation with three high-dimensional fixed effects - worker, firm, and job title - will be estimated using a nationally representative matched employer-employee data set - *Quadros de Pessoal*. The universal coverage of the employed population in the private sector in Portugal combined with the appropriate tools, create the optimal conditions for this exercise.

We found that the earnings losses are rather severe and persistent, representing 51 percent (48 percent and 52 percent) of the pre-displacement wages for firm closures (collective dismissals and individual dismissals), six years after the separation event. Those losses are largely explained by the joblessness experience of the displaced workers, during which labor earnings are absent. Wage rates also tumble for displaced workers, in comparison with non-displaced workers, amounting to a 19 percent monthly wage fall in the case of firm shut downs, and 14 percent in the case of collective dismissals, and 10 percent in the case of individual dismissals, six years after displacement.

We found that the allocation into lower-paid job titles plays the most important role in explaining the wage losses of displaced workers, accounting for half of the total average wage loss in the case of firm closure, and 54 percent in the case of collective dismissals, but not in the case of individual dismissals, where it accounts only for 11 percent of the loss. Given the way those job titles were identified, they may also reflect the bargaining power of the workers. Because job-titles contain the skill requirements of the position held by the the worker, it will also retain the hierarchical standing of the workers. Again, with the event of a displacement, a human capital will be destroyed, largely associated with the loss of his pre-displacement job-title.

Sorting into low wage firms also plays a significant role for workers displaced through firm closures, accounting for 40 percent of the total average wage loss, and 44 percent in the case of collective dismissals, and 71 percent of the loss in the case of individual dismissals. Different wage policies are favored by the existence of industry rents (due to unionization or incentive pay premiums) or the operation of wage efficiency policies. In such an environment, the worker may benefit from engaging in job search to locate the firms with more suitable (more generous) wage offers. However, with the occurrence of a displacement event, successful job searchers may loose their “job shopping” investment.

Overall, it is difficult to draw general conclusions regarding such different specific topics. Less hours will allow for the same work to be shared by more workers. However, unemployment duration is longer (not only in Europe but also in the U.S.) and flexibility is needed in terms of increasing labor mobility and therefore reduce the duration of the unemployment spell. The information that is passed to the employer through the schooling signal does not promote more job offers to the more educated unemployed. This finding raises the importance of discussing the interest of providing vocational training in order to help these workers to find a job. In terms of earnings losses of displaced workers we know that these losses are large and persistent. These losses are largely explained by the long-term joblessness. The wage losses experienced by the displaced worker are explained by the allocation into lower-paid job titles and sorting into low wage firms. Severe losses in the returns to the job-title may represent a job downgrading due to depreciation of specific human capital. Here, retraining programs may be also of some help. Losses related with the firm fixed effect may mean that a worker is moving from a “good” match to a “bad” match. If this phenomenon is pervasive, job search assistance programs and mandatory pre-notification of impeding redundancy may be justified.

Chapter 2

How a Reduction of Standard Working Hours Affects Jobs, Wages and Employment Dynamics

2.1 Introduction

This chapter investigates the effects of a working time reduction in Portugal.[1] December 1, 1996 Portugal introduced a new law on working

[1]This chapter is based on two papers that have already been published, the first called "How working time reduction affects jobs and wages" is published in the Economics Letters (Raposo and Van Ours (2010b)) and the second paper is called "How a reduction of standard working hours affects employment dynamics" and it is published in De Economist (Raposo and Van Ours (2010a)). Both papers analyze the same policy change of a reduction in standard working hours from 44 hours to 40 hours. However, since each paper had a different objective of analysis, two distinct methodologies were used. In the first paper (Raposo and Van Ours (2010b)), we investigated how working time reduction affects jobs and wages allowing for an interaction between individual and firm effects. In the second paper (Raposo and Van Ours (2010a)), we study how this mandatory reduction affected the overall level of employment through job creation and job destruction. In this chapter we describe the Portuguese policy reduction of the workweek, the theoretical framework, the literature review and the used data set, aspects common to both papers. In Section 2.7 we keep the two approaches more

hours which gradually reduced the standard workweek from 44 hours to 40 hours.[2] The main reason for implementing this mandatory reduction of working hours was to speed up convergence of Portuguese traditionally long hours of work to the European average.[3] The contribution of this chapter to the literature on working hours reduction is threefold. First, we present a more detailed analysis of potential effects. In order to assess the working hours reduction policy we analyze its effects on normal hours of work, overtime hours, hourly wages, monthly earnings and the probability of job loss. Second, we use matched worker-firm data which allow us to study the effects of working hours reduction taking firm effects into account. Finally, we are able to investigate the overall employment effects.

Employment effects of working hours reductions are not easy to establish empirically and indeed previous studies examine the impact of working hours reduction on the employment position of individuals but do not address overall employment effects. The main reason for the lack of evidence on the overall employment effects concerns the lack of information about the number of workers that find new jobs through the birth of firms. At the individual level it is rather straightforward to use workers working above the "new" standard hours before the policy change as the treated group and workers working at or below the "new" standard hours before the policy change as a control group. However, establishing the overall employment effects is a nontrivial exercise as there is no control group for firms that were created after the introduction of the policy change.

clearly separated. Therefore, in Section 2.7.1. we follow closely Raposo and Van Ours (2010b) and present the individual analysis in terms of individual jobs and wages. In Section 2.7.2. we follow closely Raposo and Van Ours (2010a) in order to investigate the overall employment effects we perform an analysis at the level of well-defined labor markets.

[2]This working hours reduction is also studied by Varejão (2005). Our study is distinct from his study because we also take potential firm effects into account.

[3]Since Portugal joined the European Union in 1986, Portugal engaged in a broad economic and political process of convergence towards the European Union average.

In the first part of the chapter we investigate how the Portuguese working time reduction affected individual jobs (Raposo and Van Ours (2010b)) while the main difference in the second part of the chapter in comparison with previous literature is that we attempt to establish the comprehensive employment effects by performing an analysis at the level of well-defined labor markets (Raposo and Van Ours (2010a)).

We assume that there is considerable regional, sectoral and firm-size variation in the share of workers who are affected by the working hours reduction. If the reduction in working hours affected employment it is likely to have had a bigger impact when the share of affected workers is high. Therefore, we can exploit the variation across labor markets to assess the impact of the workweek reduction. Accordingly, we perform an analysis on the level of labor markets defined by industry, region and firm size. This aggregate approach allows us to study job creation and job destruction as well as worker accessions and separations and thus the net employment effects.[4]

The remainder of the chapter is organized as follows. After presenting the reduction of the workweek in Portugal in section 2.2, section 2.3 discusses the economics of working hours. In section 2.4 we discuss the results from previous studies and in section 2.5 we present our data followed by a section with stylized facts. In Section 2.7 we report the results of our empirical analysis. Section 2.8 concludes.

2.2 The reduction of the workweek in Portugal

In Portugal, the 1990s were a decade with low unemployment rates; approximately 3-4% points below the EU-15 average. Portugal is considered to have a regulated and centralized labor market, with minimum wages,

[4] In a similar set-up Stewart (2002) exploits regional variation in wages across the UK to establish the impact of the introduction of the minimum wage.

strong employment protection, and collective bargaining widely applied (Cardoso (2006)). Standard working hours in Portugal are not only regulated by law but also by collective agreements at the level of the industry or the profession.[5]

In December 1996, a new law was introduced with the aim of reducing the standard workweek from 44 to 40 hours. The law was not passed as a tool to create jobs and reduce unemployment but was introduced because the newly-elected government wanted to speed up the convergence of Portuguese traditionally long hours of work to the European average (Varejão (2005)).

The new law implied first, by 1st December 1996, all workweeks above 42 hours should be reduced by 2 hours; workweeks below 42 hours but above 40 hours should meet the new standard of 40 hours per week. Second, by 1st December 1997, all workweeks still above 40 hours should meet the standard.

Workers were still allowed to work overtime, with an overtime premium of 50% for the first hour and 75% for additional overtime hours. Of course, with the reduction of standard working hours, hours in the range 40-44 became more expensive so the firms had an incentive to reduce working hours. In order to compensate firms for the reduction in working hours the new law introduced some flexibility. The reduction was implemented taking into account that the standard workweek could be defined on a 4 months average. Furthermore, the maximum number of daily working hours could be increased by 2 provided that it did not exceed 10 hours per day and 50 hours per week.[6]

[5]Usually firms cannot select themselves into a particular working hours agreement; only 5% of the workers are covered by firm-level agreements.

[6]The change in regular working hours was not a clean experiment in the sense that also the possible use of overtime hours was made more flexible.

2.3 Economics of working hours reduction

Shorter working hours may be introduced through mandatory laws or may be the result of bargaining between unions and firms (See also Boeri and Van Ours (2008)). A reduction of the workweek can be introduced for several reasons. Shorter working hours may increase the standards of living or it may be according to preferences of workers. Sometimes work sharing, i.e. reductions in the length of the working week leading to more jobs, is motivated as a tool to reduce unemployment. The idea is that if working time per worker is reduced then employment, counted as the number of workers, will increase. This of course is only true if the reduction of the working hours does not affect labor demand too much, i.e. if there is a 'lump of labor' which can simply be redistributed at no costs. In a competitive labor market it may be that workers want to organize a reduction in working hours because this would increase their marginal product of labor (Marimon and Zilibotti (2000)). The mandatory reduction of working hours would give the workers market power so they could increase their wage. Of course, individual workers would like to deviate from the agreement and work longer hours at a higher wage. in the same way as producers have an incentive to deviate from a cartel agreement. Another reason for a mandatory reduction of working hours arises when employers have monopsony power. Manning (2003) argues that in a monopsony not only the wage rate is less than the value of marginal product but the firm can also induce the worker to work more than would be optimal for the worker given the monopsony wage. In the same way as a minimum wage can be welfare improving in case of a monopsony, working hours reduction can also be welfare improving.

Whatever the reason for a working hours reduction, the question arises if there is an employment effect. From a theoretical point of view it is not obvious whether working hours reductions will increase or reduce employment. Lets assume that technology is represented by a Cobb-Douglas production function $Y = H^{\gamma}N$ where $0 < \gamma < 1$, which implies that output is proportional to the number of workers while due to physical constraints there are diminishing returns to hours of work in production. Labor costs per worker are given by

$$W = b + wH + \theta w(H - H_s)d \tag{1}$$

where H is working hours, H_s is the standard working hours, d is a dummy variable that has a value of 1 if $H \geq H_s$ and a value of zero otherwise, b represents the fixed costs per worker that are independent of working hours.[7] These are mainly the costs of hiring, firing as well as training costs, w is the hourly wage rate, θ ($\theta > 0$) is the overtime premium. Assuming a competitive product market with price equal to 1, the expression for the profits of the firm is given by

$$\Pi(H, N) = H^{\gamma}N - WN \tag{2}$$

The firm chooses H and N such that profits are maximized. The first order conditions for a maximum are:

$$\frac{\partial \Pi}{\partial N} = H^{\gamma} - b - wH_s - \theta w(H - H_s)d = 0 \tag{3}$$

$$\frac{\partial \Pi}{\partial H} = \gamma H^{\gamma-1}N - w(1 + \theta d)N = 0 \tag{4}$$

Solving these two equations we obtain the optimal number of hours as:

$$H^{*} = \frac{\gamma(b - \theta w H_s d)}{(1-\gamma)w(1+\theta d)} \tag{5}$$

When the standard number of hours is reduced the employment effects depend on the new optimal number of hours (H^{**}). We distinguish three situations:

[7] In theory, overtime should fluctuate as demand fluctuates and therefore, overtime is not exogenous. The use of overtime is not very volatile in Portugal and it is restricted to a small number of firms. In our approach we interpret the results as being conditional on the presence of overtime (d).

1. If $H^{**} < H_s^{new}$, $\frac{\partial N}{\partial H_s} = \frac{\partial H}{\partial H_s} = 0$.
2. If $H_s^{new} \leq H^{**} \leq H_s^{old}$, $\frac{\partial N}{\partial H_s} = \frac{\partial H}{\partial H_s} = 0$ or $\frac{\partial N}{\partial H_s} < 0, \frac{\partial H}{\partial H_s} > 0$.
3. If $H^{**} > H_s^{old}$, then $\frac{\partial N}{\partial H_s} > 0$, $\frac{\partial H}{\partial H_s} < 0$.

Situation 1 occurs when the optimal number of hours is below the new standard; situation 2 occurs when the optimal hours are higher than the new standard but lower than the old standard and situation 3 occurs when the optimal number of hours is higher than the old standard, i.e. the essence is Δd.

Under situation 1 the optimal number of hours is independent of the standard number of hours.[8] Therefore the change in the standard number of hours does not affect either the level of hours or the level of employment. If the new optimal hours choice is in situation 2, the hours and the employment effects depend on the overtime premium and on the fixed labor costs (see also Calmfors and Hoel (1988)). For illustrative purposes consider the following example. A firm has N workers working 42 hours at a wage w with fixed costs b. So total wage costs are $N * (42 * w + b)$. Assume that as in Portugal, the standard workweek is reduced from 44 to 40 hours while the total labor input is unaffected. Now the firm has to choose between attracting new workers in which case the total wage costs become $\frac{42}{40}N * (w * 40 + b)$. Or, the firm does not do anything, which implies that it has to pay for overtime work for which premium is 50% (as is the case in Portugal for the first overtime hour) and an overtime premium of 75% for the second hour. Then, the total wage costs become $N * (40 * w + 1.5 * w + 1.75 * w + b)$. It is straightforward to see that the firm will expand its workforce if $b < 25 * w$ while it will leave hours and employment unaffected if $b > 25 * w$. Note that this threshold is quite high as it is equivalent to half of the weekly wage. This makes it very likely that firms will follow the first strategy, i.e. reduce working hours

[8]Note that if in equation (5) there are no overtime hours $d = 0$, in which case the optimal number of working hours is given by $H^* = \frac{b\gamma}{w(1-\gamma)}$.

and expand the workforce.

In situation 3, workers already worked overtime and the hours reduction causes employment to fall. This is because the hours reduction has made the employment of a marginal worker more expensive while the price of marginal hours has not changed. Therefore, firms will reduce the production factor which became more expensive (employees) and will use more of the input of which price has not changed (hours). Heterogenous firms may react differently to a reduction in standard working hours, which makes it difficult to predict the economy-wide employment effects.

2.4 Previous studies

The most recent research on workweek reductions has studied the effects on employment and labor costs. For Germany, Hunt (1999) studies the employment effects of restrictions in hours exploring the cross-industry variation in reductions in standard hours. Starting in 1985, (West) German unions began in an uncoordinated way, to reduce the standard hours on an industry-by industry basis, in an attempt to raise employment. Hunt (1999) uses the German socio-economic panel for 30 manufacturing industries over the period 1984-1994. The question about actual hours refers to an "average" week, while the question about "last month" specifies overtime, rather than actual hours. Both reported actual hours and actual hours calculated as standard hours plus reported overtime are possible overestimates. For workers paid hourly in manufacturing, actual hours fell by 0.88-1 hour in response to a one-hour fall in standard hours. Hunt (1999) finds no positive employment effects of the gradual working time reduction that occurred in the 1980s and 1990s. Andrews et al. (2005) using German establishment-level panel data, for the period 1993-1999 estimate the policy effect on employment comparing firms employment according to the use of overtime. Instead of making use of the number of hours that individuals work each week to construct the treat and control group, they use the overtime hours worked on each firm. Andrews et al. (2005) also find no evidence of positive employment effects of working hours reduction in Germany.

For France, Crépon and Kramarz (2002) study the 1982 reduction of the workweek from 40 to 39 hours using the panel data from the French labor force survey 1977-1987 that runs in April every year. Their identification strategy explores the variation in hours worked to set up a quasi-experimental design. Hence, all those who work 40 hours in 1982 are potentially affected by the forthcoming reduction, whereas all workers employed 39 hours at that date are not affected. They find evidence that the policy change reduced hours and it didn't create jobs and increased unemployment. However, they did not measure the net effect on employment. Estevão and Sá (2008) study the further reduction of the workweek in France from 39 to 35 hours in 2000-2002 by comparing individuals employed in large and small firms, before and after the law. They match data from 1993 to 2002 from the French labor force survey with firm-level data from the French Registry of Firms (SIRENE). They find an increase in labor turnover but no effect on aggregate employment.

Skuterud (2007) presents an analysis of the Canadian province of Quebec, where the standard workweek was gradually reduced from 44 to 40 hours between 1997 and 2000. This experiment provides an interesting test for a standard workweek reduction to have a worksharing effect because the legislation contains no obligation or suggestion of any kind of wage compensation. He uses the monthly Canadian labor Force Survey over the period 1996-2002. Quebec's gradual reduction of the work week was meant to have an impact on employees in Quebec who are paid on an hourly basis and not covered by a union contract. He addresses this possibility by adding a comparison group within the jurisdiction experiencing the policy change, resulting in a triple difference estimator. Skuterud (2007) finds that, despite a 20% reduction among full-time workers in weekly hours worked beyond 40, the policy failed to raise employment at either the provincial level or within industries where hours of work were affected relatively more. Varejão (2005) investigates the effects of a 1996 working time reduction in Portugal using employer-employee matched dataset (Quadros de Pessoal) between 1995 and 1999. He does the analysis at the level of the establishment using establishment characteristics to explain differences in the effects of the workweek reduction and finds that firms' reaction to the policy is affected by the presence of minimum wage earners and the use of overtime hours.[9] Chemin and Wasmer (2009) explore geographic disparities to study the 2000 35-hour reform in France. They use the French labor force survey between 1996 and 2003. They use the historical difference of the region Alsace-Moselle as control group finding no significant impact of the 35-hour reform on employment growth. Sánchez (2010), studies the 2001 reduction of standard weekly working hours from 48 to 45 hours in Chile. He uses the EPS Panel (Encuesta de proteccion social) between 2002 and 2005 to study whether employees who worked 46-48 hours before the policy change lost their jobs more often than those not affected by the policy. He finds that this policy change did not have positive or negative employment effects.

In addition to the country studies Kapteyn et al. (2004) analyze cross-country differences in actual working hours which they interpret as work-sharing assuming that the reductions in actual working hours are driven by changes in standard hours. There is ample empirical evidence that actual hours follow standard hours. The analysis of Kapteyn et al. (2004) is based on data from 16 OECD countries from the individual labor force statistics over the time period 1960-2001. They find that work-sharing has a significant positive long-run effect on the wage rate and a positive

[9] Our study is distinct from his study because we also take potential firm effects into account. The analysis allows us to make a distinction between workers that worked 40-42 hours and workers that work more than 42 hours before the policy change. The analysis also allows us to study the effects of the working hours reduction on workers who themselves were not directly affected. In this chapter we attempt to establish the comprehensive employment effects by performing an analysis at the level of well defined labor markets. Our analysis allows us to distinguish the net employment between worker flows.

but insignificant long-run effect on employment.

In conclusion, considering the previous research, employment effects will depend on how hourly wages react to the reduction of the workweek. Under full wage compensation, although there are some country specific differences, one can expect that the employment effects will be null or negative if hourly wages increase significantly in reaction to the standard workweek reduction.

2.5 Data

We use a longitudinal data set matching firms and workers in the Portuguese economy, called Quadros de Pessoal (QP – "Lists of Personnel"). The Quadros de Pessoal data are collected annually by the Ministry of Employment through an inquiry that every establishment with wage-earners is legally obliged to fill in. Reported data cover all the personnel working for the establishment in a reference week in October. Every year QP gather information for more than two hundred thousand firms and two million workers (see Cardoso (2006) for more details). Our data cover the period 1994 until 1998. QP collects information on both standard and overtime hours done in the reference month; a posteriori we transform them into weekly hours. Standard hours are defined as the hours worked during a normal week at the going wage. Overtime hours in a week is the time worked at an overtime premium (50% for the first hour, 75% for additional hours). Monthly earnings are the monthly payments associated with the standard workweek. We use the national consumer price index to transform nominal earnings into real ones. The hourly wage is computed as the ratio of monthly earnings and standard hours of work. The worker is considered to be separated from the firm if he changes employer or leaves the sample.

In Section 2.7.1 our analysis focuses on workers, both males and females, who are full-time wage earners, i.e. workers working more than

35 hours per week.[10] We dropped individuals with missing information on normal hours of work and monthly earnings. The period before the introduction of the working hours covers October 1994 to October 1996 (See Appendix for detailed descriptive statistics). According to the way the law was implemented, the impact of the law is expected to take more than a year to completely take effect. We consider the situation in October 1997 to represent the after 1 year effects of the December 1996 change in law, while the situation in October 1998 is assumed to represent the after 2 years effects.[11]

In Section 2.7.2 in order to estimate the impact of the reduction in hours on the overall level of employment we aggregate the firms to the level of labor markets defined by industry (7 categories), region (4 categories) and size of the firm (3 categories). Thus we perform our analysis at the level of 84 labor markets.[12]

2.6 Stylized facts

Table 2.1 shows that in the period October 1994-1996 on average 22% of the Portuguese workers had a standard workweek between 40 and 42 hours, while 30% had a workweek of more than 42 hours. So, half of the Portuguese workers were not affected by the reduction in standard working hours because through collective agreement their standard workweek was already below the new standard. By October 1997 the percentage of workers working more than 40 hours decreased to 38 and by October

[10]The main reason is that workers working less than 35 hours may have a different attachment to the labor market.

[11]In section 2.7.1 we exploit a 10% random sample using the Stata sampling procedure "sample2". This procedure allows the creation of a random sample by clusters of observations. Once an individual is randomly chosen all observations of this individual are sampled. Thus a sample with the original panel characteristics of the population is created.

[12]For some of these labor markets we did not use information from every year. We removed some outliers, where the change in job creation rate and employment growth was strongly negative.

1998 only 9% of the workers worked more than 40 hours.

Table 2.1: **Proportion of workers in each hour category; standard working hours**

	October			Δ	Δ
	1994-1996	1997	1998	1994/6-97	1997-98
<35	11	8	11	-3	3
35-40	37	54	80	17	26
40-42	22	23	8	1	-15
>42	30	15	1	-15	-14
Total	100	100	100	0	0

Given that in period t an individual is working in a certain hour category, in the next period, he might continue working in the same hour category, he might change hour category and work overtime or not or finally, he might lose his job. Table 2.2 shows the proportion of workers in all three situations. Although the majority of individuals do not change their hour category, the fraction of individuals that change hour category is large specially in the categories over 40 hours. There is a large fraction of individuals that lose their job (21 to 29%), specially in the categories over 40 hours. However, the probability that an individual will lose his job does not seem to be related with the legislation change.

We observe a clear change in hours' categories, specially after 1996 there is a clear decrease in the number of hours worked. From part c and d of table 2.2 we see that after 1996, the treated group has a clear tendency to go more to lower hour categories and go less to higher hour categories. Thus, the policy change clearly affected the hour distribution of the individuals in the treated group.

According to the Portuguese National Institute (INE), firms pay overtime hours mainly up to 10 hours per week. In 1996 there was a clear increase in the use of overtime hours and the effect lasted up to the early years of 2000. Even being frequently used the proportion on the overall

Table 2.2: **Transition rates given usual hour category in t**

t		$t+1$ Loose job	< 35	35-40	41-42	> 42	Total
a. 1994-1995	35-40	22	4	61	12	1	100
	41-42	25	2	11	59	3	100
	>42	29	3	10	25	33	100
b. 1996	35-40	21	3	70	6	0	100
	41-42	24	2	30	43	1	100
	>42	25	2	14	45	14	100
c. △	35-40	-1	-1	9	-6	-1	0
	41-42	-1	0	19	-16	-2	0
	>42	-4	-1	4	20	-19	0
d. △△	41-42	0	1	10	-10	-1	0
	>42	-3	0	-5	26	-18	0

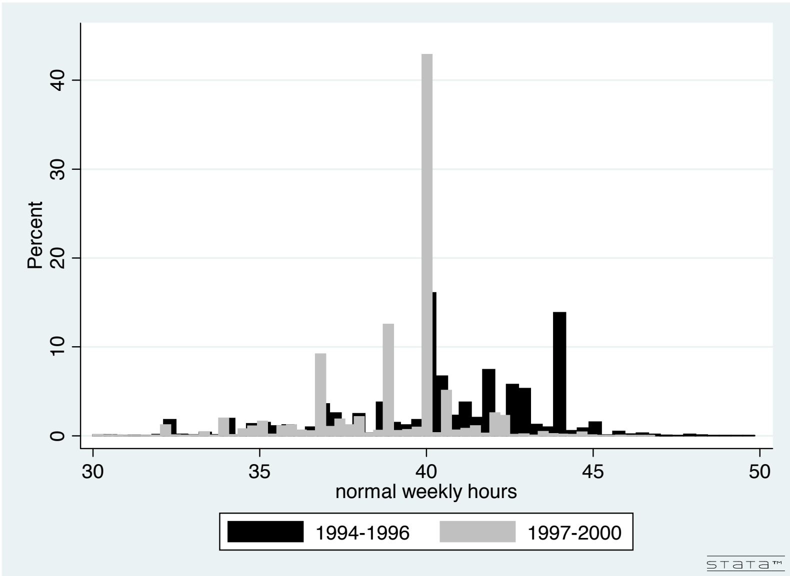
Notes: Panel c is the difference between panels a and b respectively for each hour category. Panel d uses panel c to compute the difference respectively between 41-42 and more than 42 hours category and the 35-40 hour category.

use of payed overtime hours of work decreased significantly in our sample. The histograms in figure 2.1 (panel a and b) show the proportion of full-time workers employed on each number of hours. The fraction of workers working more than 40 hours clearly decreased and almost disappeared after 1997.

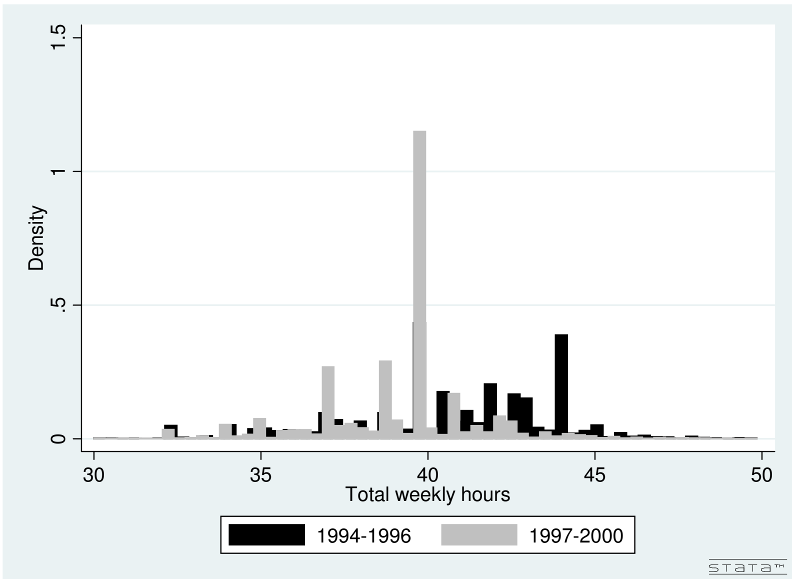
Besides checking the impact of the policy on the individual level it is interesting to include the firm characteristics especially, because it is also a firm decision to react to the policy and therefore it does not matter to see the worker individual perspective without given a context on the firm level. First, we must understand some stylized facts from firm dynamics in Portugal. The firm flows in Portugal during the 1990s are very high reflecting a firm turnover of around 35% every year.[13] This indicates that one third of the firms every year is either starting or ceasing its activity. The entry rate of firms increased between 1996 and 1997 (3%). In opposition the rate of firm exits remained high but decreased by 2%

[13]The average firm size between 1986 and 2008 is 11 workers. And it has decreased from 18 in 1986 to 9 workers in 2008.

Figure 2.1: Histogram of hours worked



(a) Histogram of usual hours worked



(b) Histogram of usual plus overtime hours worked

in the same period.

We are interested in the behavior of certain types of firms in what concerns their reactions in terms of use of overtime, firing and hiring. We already explained that the use of overtime might have affected the reaction of firms towards the policy. After the policy change we saw that certain overtime hours became more expensive than before. Nonetheless, some firms might need to continue using overtime or other firms whose workers were working between the old and the new standard might start using overtime. We find that the proportion of individuals working overtime inside each firm is around 30% and it was not affected by the policy change in 1997. The proportion of firms that have between 0 and 10% of their workers doing overtime is 9% before the policy change and decreased to 7% in 1997. There is not much variation in the use of overtime which seems to indicate that the policy change did not affect each firms decision about the use of overtime. The proportion of firms using overtime before 1996 was not very significant (5%) and it was not affected by the policy change.

Table 2.3: Means of variables (annual percentages)

	1994-95	1995-96	1996-97	1996-98
Δe	3.9	3.7	6.6	5.2
JC	18.7	18.6	20.4	19.4
JD	14.8	14.9	13.8	14.2
WA	37.1	36.6	38.6	37.5
WS	33.1	32.9	32.2	32.5

Notes: Δe represents the net employment employment between two subsequent dates. JC, JD, WA and WS represents respectively, job creation, job destruction, worker accession and worker separation. October data.

Table 2.4: **Job flows and worker flows; 1994-1998**

	Firm New (1)	Firm Expansion (2)	Firm Contraction (3)	Firm Closure (4)	Total Job Creation (1+2)	Total Job Destruction (3+4)	Net Employment	Worker Accession (5)	Worker Separation (6)
1994-95	8	9	6	7	17	14	3	33	30
1995-96	7	8	7	8	15	15	0	31	31
1996-97	10	9	6	7	18	13	5	35	30
1997-98	9	9	6	9	18	15	3	34	31

Notes: Change in employment between two subsequent dates as percentage of the average employment at these two dates; October data.

Table 2.3 shows the means of the variables we use in the aggregate analysis. We define job and worker flows as usual (see Appendix). Table 2.4 shows that total job creation in the period October 1994-95 was 17% while in the year thereafter it was 15%. Job creation increased to 18% in the period October 1996-97 and October 1997-98. On the other hand total job destruction in the period October 1994-95 and October 1995-96 was around 14-15% while it decreased to 13% in the period October 1996-97. In the year thereafter job destruction increased again to 15%. Thus, immediately after the policy was implemented in December 1996 job creation increased and job destruction decreased causing net employment to increase 5%. In the year thereafter job creation was constant while job destruction increased somewhat, causing employment to grow 3%. Of course, these developments in job flows and worker flows may have been affected by the working hours reduction, but they may also be influenced by changes in the Portuguese economy. In an economy known to be characterized by very high levels of firm creation and firm closures (Mata and Portugal (1994)) it is not strange to see that after 1996, job creation increased mainly through new firms. The main reason behind the decrease of the job destruction is not so clear, firm closure or firm contraction. The last two columns in Table 2.4 show that worker accessions fluctuate more than worker separations. It is also clear that worker turnover rates are high. Every year about one third of all Portuguese workers leave their job and find a new job.[14]

[14] Blanchard and Portugal (2001) argue that those high turnover rates in Portugal are related to the small size of firms in Portugal.

2.7 Empirical analysis

2.7.1 Individual worker empirical analysis

Exploratory analysis

We analyze the impact of the reduction of the work week assuming that the policy change resembles a natural experiment. The treated group consists of all individuals who worked more than 40 hours in October 1996; the control group consists of workers who worked 35-40 hours in October 1996.[15]

The new law implied first, by 1st December 1996, all workweeks above 42 hours should be reduced by 2 hours; workweeks below 42 hours but above 40 hours should meet the new standard of 40 hours per week. Second, by 1st December 1997, all workweeks still above 40 hours should meet the standard. Note that the data in Quadros de Pessoal is collected in October. Thus, looking at Equation (6) we see how the empirical results after 1 and 2 years can be interpreted as responses to the first and second policy change. To analyze the effects of the working week reduction we estimate the following equations:

$$\Delta y_{it} = \alpha_t + \beta x_{it} + (\delta_1 h_{4042,i} + \delta_2 h_{42p,i}) d_{96,t} + (\delta_3 h_{4042,i} + \delta_4 h_{42p,i}) d_{97,t} + \epsilon_{it} \quad (6)$$

where Δy, the dependent variable, represent changes in standard hours, overtime hours, hourly wages, monthly wages and job separation rate (defined by the instantaneous probability of leaving the job), for individual i in the period from October in year t to October in year $t+1$. Furthermore, the α_t represent calendar year fixed effects, x represents a vector of personal characteristics, h_{4042} and h_{42p} are dummy variables representing

[15] To exploit the natural experiment character of the working time reduction we focus on workers that are close to the "threshold" of 40 hours per week and ignore workers on part-time jobs (less than 35 hours).

working hours categories (40-42 hours and >42 hours per week), $d_{96,t}$ and $d_{97,t}$ are dummy variables for the post reform period, respectively, first year and second year. $d_{96,t}$ measures the effect between october 1996 and october 1997 and therefore it can be interpreted as a response to the first policy change (possibly with some lag). $d_{97,t}$ measures the effect between october 1997 and october 1998 and therefore it can be interpreted as a response to the second policy change. Finally, β is a vector of parameters, the δ's are also parameters while ϵ is an error term.

In a policy change evaluation it is fundamental to rule out bias from self-selection as individuals are not randomly assigned to the treatment. The individual's decision to work more than 40 hours in October 1996 is independent from a possible outcome or the treatment effect. Proper identification in a difference in difference framework relies on two important assumptions. The first is the assumption of common time effects across groups, and the second assumes there are no systematic composition changes for each group. The group of non-treated is statistically equivalent to the treated group in all dimensions except treatment status (Blundell and Dias (2002)). Thus, the control group is a good counterfactual of what would have happened to the treated group in the absence of the change in the law. Our empirical design already ensures these assumptions along two dimensions. First, the common trends assumption presumes that treated and controls experience common trends or, in other words, the same shocks. Second, the empirical model that we explore controls for observed and unobserved permanent heterogeneity, which enhances the comparability between the two groups.

The working hours categories are defined on the basis of the situation in October 1996, just before the introduction of the working time reduction. Assuming that the workers working less than 40 hours per week were not affected, the general calendar time effects are represented by the calendar year dummies and assuming that the differential development of the affected workers is represented by the working hours categories, the parameters δ_1 and δ_2 of the interaction terms represent the treatment effects. The relevant parameter estimates are presented in the upper part of Table 2.5.

As expected standard working hours go down substantially. Overtime hours increase in the first year, but in the second year they are approximately constant. Apparently, the initial reduction of standard working hours is partly compensated by an increase in overtime hours although this effect is small. Hourly wages for workers affected increase, leaving monthly earnings approximately constant.[16] Somewhat surprisingly the affected workers in the category 40-42 hours have a lower probability to lose their job than non-affected workers. This may be explained by the flexibility that firms could use on this group of workers.

So far, we ignored firm information. However, it might be that the treatment effects are influenced by the firm share of workers that worked more than 40 hours per week. After all, firms' costs of the working hours adjustment increase with the share of workers affected. To investigate this possibility we add to equation (1) a number of interaction terms:[17]

$$\Delta y_{it} = \alpha_t + \beta x_{it} + \{\zeta_1.n_i + (\delta_1 + \omega_1 n_i)h_{4042,i} + (\delta_2 + \omega_2 n_i)h_{42p,i}\}d_{96,t}$$
$$+(\zeta_2.n_i + \delta_3 + \{\omega_3 n_i)h_{4042,i} + (\delta_4 + \omega_4 n_i)h_{42p,i}\}d_{97,t} + \epsilon_{it} \qquad (7)$$

where n_i represents the share of workers in the worker i's firm who worked more than 40 hours in October 1996. To the extent that the ω's differ from zero the composition of the workforce affects the treatment effect. The ζ parameters represent "spillover" effects on workers who themselves were not directly affected. As shown in the lower part of Table 2.5, these workers increase the number of hours they work if the proportion of af-

[16]This was also found by Hunt (1999) in Germany and is attributed to the influence of unions demanding no decline in monthly earnings.

[17]And, we also added to the equation $n_i, n_i.d_{96,t}, n_i.d_{97,t}$, to make sure that the ω-parameters represent the treatment effects.

fected workers in the firm is large. These workers also have a bigger probability to separate from their firm when the proportion of affected workers is larger.

Table 2.5: **Parameter estimates of the individual analysis**

		Standard hours	Overtime hours	Hourly wage (%)	Monthly earnings (%)	Job separation (%)
a. Personal characteristics						
Effects after 1 year						
h_{4042}	δ_1	-1.54 (36.4)**	0.06 (2.7)**	3.66 (13.5)**	-0.53 (1.8)*	-4.95 (13.6)**
h_{42p}	δ_2	-3.57 (95.3)**	0.08 (4.7)**	8.26 (34.9)**	-0.56 (2.2)**	0.01 (0.0)
Effects after 2 years						
h_{4042}	δ_3	-0.90 (18.6)**	0.05 (2.2)**	2.93 (11.5)**	0.43 (1.5)	-5.73 (16.4)**
h_{42p}	δ_4	-2.38 (55.2)**	0.02 (1.4)	5.88 (25.9)**	0.05 (0.2)	-0.01 (0.3)
$\overline{R}^2$		0.091	0.0003	0.014	0.004	0.040
b. Personal and firm characteristics						
Effects after 1 year						
h_{4042}	δ_1	-2.53 (20.5)**	0.02 (0.3)	6.06 (7.2)**	-1.44 (1.6)	-3.64 (3.4)**
$h_{4042}.n$	ω_1	-0.01 (0.1)	-0.01 (0.1)	-2.39 (2.1)**	-1.47 (1.2)	-3.33 (2.3)**
h_{42p}	δ_2	-4.47 (36.0)**	0.00 (0.1)	9.96 (13.2)**	-1.61 (1.9)*	0.31 (0.3)
$h_{42p}.n$	ω_2	-0.20 (1.2)	0.03 (0.4)	-1.34 (1.3)	-1.39 (1.2)	-1.97 (1.5)
$h_{3540}.n$	ζ_1	1.89 (19.9)**	0.09 (2.4)**	-1.44 (2.3)**	3.61 (5.3)**	1.83 (2.3)**
Effects after 2 years						
h_{4042}	δ_3	0.01 (0.1)	-0.11 (1.7)*	0.78 (1.1)	0.76 (0.9)	-1.27 (1.3)
$h_{4042}.n$	ω_3	-2.42 (12.6)**	0.21 (2.3)**	3.21 (3.3)**	-3.63 (3.2)**	-10.18 (7.3)**
h_{42p}	δ_4	-1.92 (14.2)**	-0.02 (0.5)	5.38 (8.1)**	0.66 (0.8)	5.24 (6.0)**
$h_{42p}.n$	ω_4	-1.73 (9.3)**	0.04 (0.6)	0.75 (0.8)	-4.02 (3.7)**	-11.09 (8.9)**
$h_{3540}.n$	ζ_2	1.49 (15.1)**	0.03 (0.8)	-0.14 (0.3)	4.30 (6.9)**	5.11 (6.9)**
$\overline{R}^2$		0.094	0.0003	0.014	0.004	0.040
F-statistic		168.34**	2.06**	20.45**	9.05**	18.19**

Notes: Ordinary least squares; first four columns based on 415,863 observations, the fifth column based on 536,997 observations; parameter estimates of control variables are not represented; control variables include calendar year fixed effects, working hours dummies for categories 40-42 hours and more than 42 hours, industry (10 categories), region (7 categories), education (8 categories), wage (5 categories)(not included in the wage and earnings regressions), size of firm (4 categories) and tenure. The estimates in the lower part of the table also contain firms' share of workers working more than 40 hours per week and the interaction terms between this share and calendar time fixed effects. The population includes all full-time workers in the private sector working between 35 and 50 hours. Absolute t-statistics based on robust standard errors in parentheses; The F-statistic concerns a comparison of the estimation results in the lower and the upper part of the table; a **/* indicates that the coefficient is different from zero at a 5%/10% level of significance.

Table 2.5 also shows that ω_1 and ω_2 are often insignificantly different from zero indicating that in the first year the composition of the workforce is not very important. However, since the other ω-parameters often differ significantly from zero, in the second year the treatment effect is influenced by the workforce composition.

Table 2.6 gives an idea of the size of the firm effects. The treatment effects are calculated on the basis of the parameter estimates of the lower part of Table 2.5 and concern the marginal effects evaluated when 25 and 50% of the firms workforce worked more than 40 hours before the policy change. From these calculations we draw two conclusions. First, the treatment effects are bigger – in absolute terms – for workers who worked more than 42 hours per week, with one exception, the job separation rate. Workers in the category 40-42 hours are less likely to separate than workers working fewer or more hours. Our second conclusion concerns the firm effects. Most of the treatment effects do not depend on the share of workers working 40 hours or more. Firm effects are significant but quantitatively not very important. The only exception concerns the job separation rate in the second year. Somewhat surprisingly, these are more favorable the higher the share of workers working more than 40 hours. Our interpretation of this phenomenon is that there are negative spillover effects affecting workers that worked less than 40 hours per week.

2.7.2 Aggregate empirical analysis

Set-up of the analysis

By using market level data we can take the creation of new firms in a particular market into account because they are part of employment creation in that market. The way these labor markets are affected by the working hours reduction depends on how many workers are affected within these labor markets.[18] As a definition of policy intensity we use a variable n, defined as the share of affected workers inside each labor

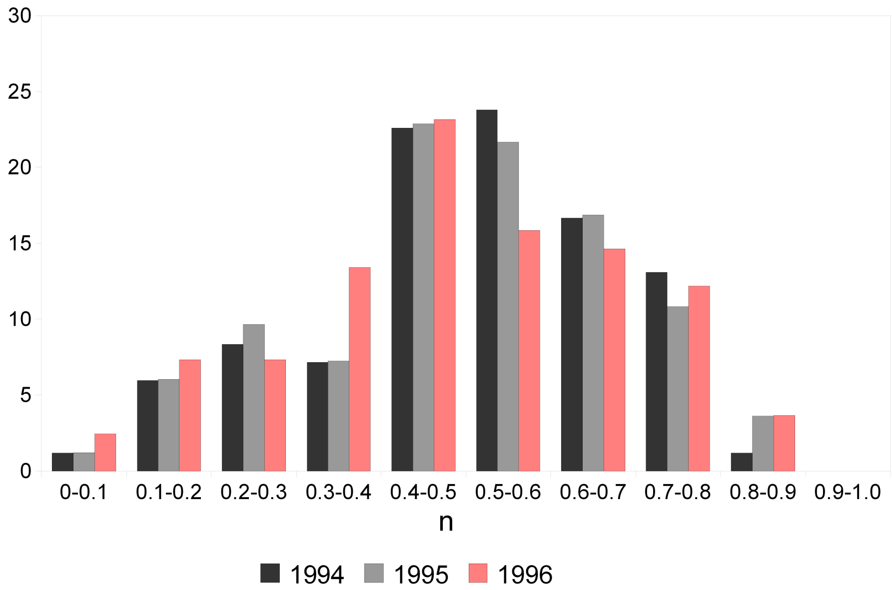
[18] Obviously, the hourly wage may have been affected by the reduction of standard working hours. However, this does not affect our reduced form analysis, in which we relate the change in standard working hours to the employment effect without attempting to distinguish between the various determinants of this change.

Table 2.6: **Treatment effects of the individual analysis**

Weekly working hours	35-40		40-42		>42	
% of workers affected	25	50	25	50	25	50
	(1)	(2)	(3)	(4)	(5)	(6)
Effects after 1 year						
Standard hours	0.5	0.9	-2.5	-2.5	-4.5	-4.6
Overtime hours	0.0	0.0	0.0	0.0	0.0	0.0
Hourly wage (%)	-0.4	-0.7	5.5	4.9	9.6	9.3
Monthly earnings (%)	0.9	1.8	-1.8	-2.2	-2.0	-2.3
Job separation (%)	0.5	0.9	-4.5	-5.3	-0.2	-0.7
Effects after 2 years						
Standard hours	0.4	0.7	-0.6	-1.2	-2.4	-2.8
Overtime hours	0.0	0.0	-0.1	-0.0	-0.0	0.0
Hourly wage (%)	-0.0	-0.1	1.6	2.4	5.6	5.8
Monthly earnings (%)	1.1	2.2	-0.1	-1.1	-0.3	-1.4
Job separation (%)	1.3	2.6	-3.8	-6.4	2.5	-0.3

Notes: The percentage of workers affected concerns the workers that worked more than 40 hours per week in October 1996; the calculations are based on the parameter estimates of the lower part of Table 2.5.

Figure 2.2: **Market level distribution of the share of workers working more than 40 hours per week; October 1994-96**



Notes: n is the share of affected workers inside each labor market working more than 40 hours at the relevant October dates. 0-0.1 means 0 to 10% of the total number of workers are affected.

market working more than 40 hours at the relevant October dates. The distribution of n in the period October 1994 to October 1996, shortly before the working hours reduction, is presented in Figure 2.2. Clearly the policy intensity varies a lot between the different labor markets. Over time there are some changes in the distribution but by and large the distribution of n in various years looks very much alike.

To analyze the year 1 effects of the working week reduction in labor market k represented by industry, region, size we estimate the following equation:

$$y_{kt} = \alpha_t + \alpha_k + \beta n_{kt} + \delta n_{kt}.d_p + \epsilon_{kt} \tag{8}$$

The dependent variables are job creation rate (JC), job destruction rate (JD), worker accession rate (WA) and worker separation rate (WS) and change in employment (Δe) from t to $t+1$, where t runs from October 1994 to October 1996. Furthermore, the α_t represents calendar time fixed effects, the α_k represent time-invariant labor market fixed effects, d_p represents a dummy variable for the post reform period, and n represents the share of individuals that worked more than 40 hours in October of year t. The main parameter of interest is δ, representing the treatment effect. Finally, ϵ represents an error term.

Exploratory analysis

To give an idea about the relationship between the share of workers working more than 40 hours per week and employment growth, job creation and job destruction Figure 2.3 presents an exploratory analysis. The horizontal axis shows the share of workers working more than 40 hours per week in October 1996, shortly before the mandatory reduction in the standard working week was implemented. The vertical axis shows changes in the period October 1996 to October 1997 in employment growth (panel a), job creation (panel b) and job destruction (panel c).

As shown in panel a of Figure 2.3 the higher share of 40+ hours workers, the higher the change in employment growth. The slope of the straight lines in Figure 2.3 represent an estimate for δ.[19] Indeed, the slope

[19]Note that if we take first differences of equation 8 over the period 1995-96 we find:

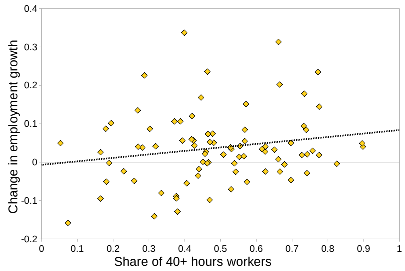
$$\Delta y_{k,95-96} = \alpha_{96} - \alpha_{95} + \beta(n_{k,96} - n_{k,95}) + \delta n_{k,96} + \epsilon_{k,96} - \epsilon_{k,95} \tag{9}$$

such that if n didn't change too much between 1995 and 1996 we find:

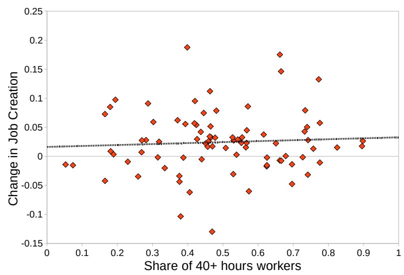
$$\Delta y_{k,95-96} \approx \alpha^* + \delta n_{k,96} + \epsilon^* \tag{10}$$

In a linear regression we find for δ (absolute t-statistics based on robust standard errors): panel a: 0.090 (1.8), panel b: 0.017 (0.6), panel c -0.074 (2.2).

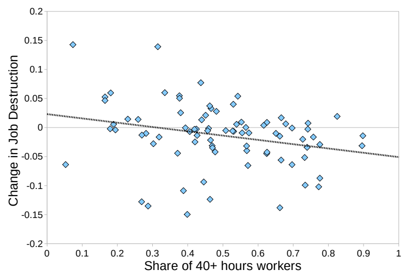
Figure 2.3: Changes in employment growth, job creation and job destruction; 1996-1997



(a) Employment growth



(b) Job creation



(c) Job destruction

is positive in panel a indicating that the larger the share of workers involved in the reduction of the standard workweek, the higher employment growth.

Panel b shows that there is no such relationship with job creation, while from panel c it is clear that job destruction is affected by the reduction of the standard workweek: the larger the share of workers involved in the reduction of the standard workweek, the lower the change in job destruction.

Parameter estimates

The parameter estimates for δ from equation 8 estimated over the period 1994-97 are presented in the first column of Table 2.7. As shown, the change in employment is significantly affected. The higher n, the higher employment growth. Conditional on the other characteristics of the labor market, an average labor market with an n of 0.5 experiences an employment growth of almost 5%. As shown, job creation and worker accessions are not affected by the reduction of the standard workweek. However, job destruction and workers separations are negatively affected. Apparently, labor markets confronted with a reduction of the standard workweek reduce job destruction and thus increase employment. This would be in line with predictions from the theoretical model. Limiting the estimation period to 1995-97 hardly affects the parameter estimates (column 2).[20]

Replacing y_{kt} for $t = 1996$ in equation 8 by the averages for the period 1996-98 we also estimated the year 2 effects of the working hours reduction. The parameter estimates are presented in the third and fourth column of Table 2.7. The results are very much the same as before.

The market level analysis allows us to make a distinction between worker accessions to new firms and worker accessions to firms that survive. Similarly, we can make a distinction between worker separations from firm closures and worker separations from surviving firms. Table 2.8 also shows how the working hours reductions affects the flow of workers to and

[20]This is evidence that the common trend assumption holds.

Table 2.7: Baseline estimates of the aggregate analysis

	1 year effect		2 years effect	
	1994-97	1995-97	1994-98	1995-98
	(1)	(2)	(3)	(4)
Δe	0.097 (2.1)**	0.125 (2.2)**	0.091 (1.8)*	0.110 (1.8)*
JC	0.013 (0.5)	0.012 (0.4)	0.006 (0.2)	-0.002 (0.1)
JD	-0.084 (3.1)**	-0.113 (3.3)**	-0.085 (2.4)**	-0.113 (2.5)**
WA	0.020 (0.6)	0.030 (0.9)	-0.004 (0.1)	-0.007 (0.2)
WS	-0.082 (2.3)**	-0.096 (2.1)**	-0.097 (2.2)**	-0.117 (2.2)**
Observations	249	165	250	165
Labor markets	84	84	84	84

Notes: All estimates also have the share of 40+ hours workers (n) as explanatory variable in addition to labor market fixed effects (84) and calendar period fixed effects (3); absolute t-statistics based on robust (cluster) standard errors in parentheses (Bertrand et al. (2004)); a **/* indicates that the coefficient is different from zero at a 5%/10% level of significance.

Table 2.8: Sensitivity analysis; distinguishing between surviving firms and firms being born/dying

	1 year effect		2 years effect	
	1994-97	1995-97	1994-98	1995-98
	(1)	(2)	(3)	(4)
	Firms being born and dying			
Δe	0.089 (2.6)**	0.112 (2.6)**	0.061 (1.8)*	0.083 (2.1)**
WA	0.045 (1.9)*	0.048 (1.8)*	0.019 (0.9)	0.019 (0.8)
WS	-0.044 (2.7)**	-0.064 (2.7)**	-0.042 (1.9)*	-0.064 (2.2)**
	Surviving firms			
Δe	0.008 (0.3)	0.013 (0.4)	0.030 (1.1)	0.027 (0.8)
WA	-0.025 (0.9)	-0.019 (0.7)	-0.024 (0.8)	-0.026 (0.8)
WS	-0.038 (1.4)	-0.032 (1.0)	-0.055 (2.1)**	-0.052 (1.7)*
Observations	249	165	250	165
Labor markets	84	84	84	84

Note: see footnote Table 2.7.

from these different types of firms. The working hours reduction mainly affects worker flows to new firms and from firm closures. Conditional on the other characteristics of the labor market, firms being born and dying on an average labor market with an n of 0.5, experience an employment growth of almost 5% after 1 year, where half of this effect comes from worker separations and half comes from worker accessions. After 2 years the effect on net employment is positive but smaller (3.5%) and it comes mainly from the reduction of worker separations. The effects to and from surviving firms are much smaller after 1 year, but after 2 years the effects are very similar.

2.8 Conclusions

The reduction of the maximum standard workweek from 44 to 40 hours directly affected about half of all workers in Portugal since they had a standard workweek of more than 40 hours. We study this working hours reduction that was introduced in Portugal in 1996.

Initially, the reduction of working hours was compensated by the use of overtime. Hourly wages of the affected workers increased, reducing their monthly earnings only slightly. Workers in the category 40-42 hours were less likely to separate from their firm. Due to spillover effects at the firm level the working hours reduction also affected workers working less than 40 hours per week. Our analysis allows us to say something about how the working time reduction affected workers who themselves where not directly affected because they already worked below 40 hours, but who were indirectly affected when part of the workforce at their firm worked more than 40 hours. We show that indeed these workers were affected. We attribute this to the overall cost of the working hours adjustment for the firm which increase with the share of workers affected. Therefore the estimates presented are in fact a lower bound of the true effects because due to spillover effects also the control groups may have been affected.

Previous empirical studies suggest that reductions of standard working hours do not have positive employment effects. However, previous studies only measure partial employment effect while in our study we consider overall employment effects. In our analysis we exploit regional, sectoral and firm-size variation in the share of workers who were affected by the working hours reduction. A working hours reduction is likely to have had a bigger impact when the share of affected workers was high. To investigate this we perform an analysis on the level of labor markets defined by industry, region and firm size. We find evidence that the working hours reduction had a positive effect on employment through a fall in job destruction. We can only speculate about the reason why reducing standard working hours in Portugal increased employment whereas in other countries no such effects occurred. Most likely, the increased flexibility in the use of the standard workweek made it easier to adjust the workforce at the intensive margin rather than at the extensive margin. To the extent that this reduced labor costs, job destruction was reduced, causing positive employment effects. The reduced labor costs did not stimulate job creation which may have to do with the strict employment protection in Portugal.

Previous studies found null or negative employment effects due to the adjustment of hourly wage. We add to the literature because we find evidence that under full wage compensation and if it is difficult for firms to adjust the workforce one can expect positive employment effect via a decrease in job destruction.

2.9 Appendix

Appendix A - Measuring job flows and worker flows

We define job and worker flows as usual (Davis and Haltiwanger (1999)). We denote the level of employment at firm j in period t as e_{jt}: the average number of employees at the start and the end of the period. We denote the change of employment at firm j during period t as Δe_{jt}; the change is calculated as the difference between the number of workers at the end of the period and the number of workers at the beginning of the period. The job destruction rate (JD) in period t in the universe of firms S is specified as:

$$JD_t = \sum_{j \epsilon S_t^-} (|\Delta e_{jt}|) / \sum_{j \epsilon S_t} e_{jt} \tag{11}$$

where S_t^- represents the subset of firms with $\Delta e_{jt} < 0$. In the same way job creation rate (JC) is defined as:

$$JC_t = \sum_{j \epsilon S_t^+} (\Delta e_{jt}) / \sum_{j \epsilon S_t} e_{jt} \tag{12}$$

where S_t^+ represents the subset of firms with $\Delta e_{jt} > 0$. These measures of job flows underestimate the true values of gross job destruction and creation. Even if at the level of an individual firm employment change equals zero there might be some job creation and job destruction going on. With heterogeneous workers, jobs and firms making the distinction between job and worker flows is fundamental.

If F is the number of workers that left the firm in a particular period, and H denotes the number of workers that entered the firm in that period, worker separations (WS) in period t in the universe of firms S is defined as:

$$WS_t = \sum_{j \epsilon S_t} (F_{jt}) / \sum_{j \epsilon S_t} e_{jt} \tag{13}$$

and worker accessions (WA) in period t in the universe of all firms S equals:

$$WA_t = \sum_{j \epsilon S_t} (H_{jt}) / \sum_{j \epsilon S_t} e_{jt} \tag{14}$$

By definition, it holds that:

$$JC_t - JD_t = WA_t - WS_t = \Delta e_t \tag{15}$$

A surviving firm is a firm that is reported in our data both in 1996 and in the current year (t). The birth of a new firm is reported if it is the first time this firm is reported in our data. We consider there exists a firm closure if a firm is reported as having gone out of business at time t if that year is the first year it fails to report.

Appendix B

Table 2.9: Descriptive statistics

	All	h40p	h3540
Age (years)	36	35	37
Male	62	62	62
Standard hours	40	41	38
Overtime hours	0	0	0
Monthly earnings (2008 euros)	782	650	956
Hourly wage (2008 euros)	1.45	1.15	1.83
Tenure (in months)	108	102	116
Education (percentages)			
Less than basic school	3	3	2
Basic school	41	49	30
Lower Secondary	22	26	18
Upper Secondary	15	11	20
Bachelor	13	8	20
College	1	1	3
Master	3	1	6
PhD	2	1	2
Firm size (percentages)			
Less than 19	26	30	21
19 - 89	26	28	23
90 - 550	25	25	25
More than 550	23	16	31
Industry (percentages)			
Food, textiles	1	1	1
Mineral products	1	0	1
Manufacturing	38	45	30
Electricity	1	2	0
Construction	8	6	10
Trade	22	24	18
Transports	8	6	11
Banking, insurance	8	2	15
Business services	7	6	9
Other services	6	7	5
Region (percentages)			
North	35	43	25
Algarve	2	3	2
Center	17	18	14
Lisbon	38	28	53
Inland	4	4	4
Azores	1	2	1
Madeira	2	2	1
No. Observations	415863	236282	179581

Notes: This table reports summary statistics (mean) used in the analysis to construct the sample. The second column shows statistics computed using all workers and on the third and fourth columns they are computed using the sample of individuals working more than 40 hours and less than 40 hours respectively. The units are explained in front of the variables while gender, education and industry are shown as a percentage.

Chapter 3

Joblessness

3.1 Introduction

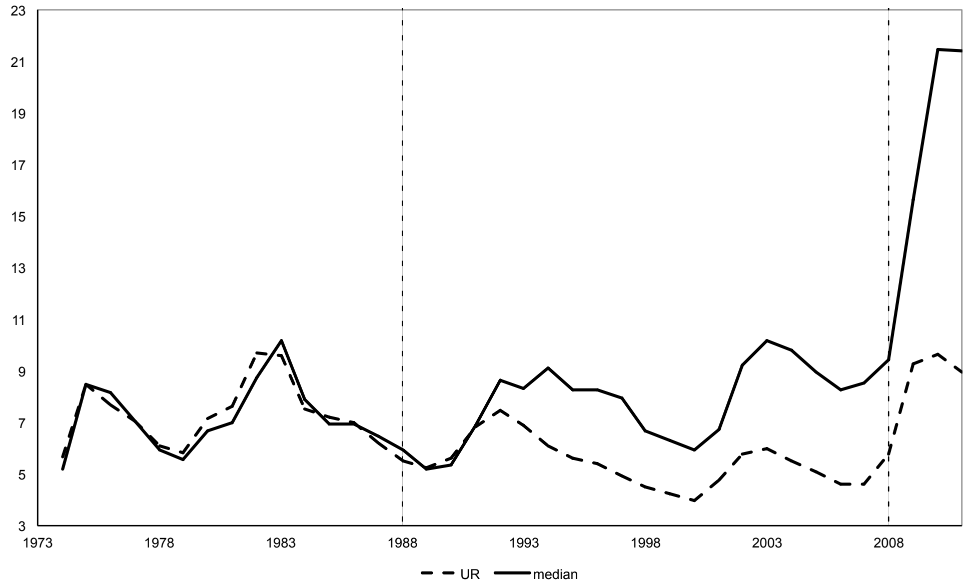
The U.S. labor market has changed significantly since 1985 up until the financial crisis.[1] Unemployment rates shifted downward to 5 percent or below. This trend toward lower unemployment rates was largely driven by lower unemployment inflows (Hall (2006)) and higher job finding probabilities (Shimer (2012)). Concurrently, however, mean elapsed unemployment duration surprisingly trended up. Indeed, average unemployment duration reached 18 weeks in 2008. Figure 3.1 shows that the Current Population Survey (CPS) series of unemployment rates and median elapsed weeks of unemployment used to be very well-aligned until the end of the eighties. The two series began diverging significantly in the early nineties and the gap has widened ever since (see Figure 3.1). In a sense, the American job market, with these rising unemployment rates and unemployment duration, resembles more the European.

The striking evolution of unemployment in the United States has not gone un-noticed. A number of studies have examined the question of why

[1]This chapter is based on a joint paper with Pedro Portugal and José António Machado that has been published as a working paper, called "Joblessness" (Raposo et al. (2010)).

the unemployment duration became so much longer (Baumol and Wolff (1998); Valletta (1998); Abraham and Shimer (2001); Juhn et al. (2002); Mukoyama and Sahin (2009); and, Aaronson et al. (2010)).

Figure 3.1: **Unemployment rate and median unemployment duration**



Source: Labor Force Statistics from the Current Population Survey. Units of the graph: Unemployment rate in % and median unemployment duration in weeks.

Explanations for the recent rising trend of average unemployment duration rely either on the compositional changes of the labor force or, more fundamentally, on the emergence of some economic mechanisms.[2] Examples of the former explanation include Abraham and Shimer (2001). They use CPS data for the period 1968 and 2000 to estimate labor-market-transition rates to show that the aging of the baby-boom generation and the increased labor force attachment of women contributed to the ob-

[2]The influence of methodological changes in the CPS surveys has also been studied (see, e.g., Abraham and Shimer (2001)).

served enlarged share of long-term unemployed. According to the authors the growing stability of women's employment is also determinant to understand why their unemployment rates decreased and unemployment duration has increased. Aaronson et al. (2010) examine aggregate data based on interrupted spell durations from the CPS, and also find that the unemployment duration increase can be explained by demographic changes (age and gender), i.e. older workers have longer durations and there are now more older workers. Juhn et al. (2002) use employment data from the 1968-2001 annual demographic files that supplement the CPS. They estimate equations for unemployment transitions and unemployment durations and they claim that joblessness among less-skilled men has taken the form of time spent out of the labor force rather than unemployment *per se*.[3] Valletta (1998) uses data from the CPS accounting for the 1994 CPS redesign to estimate unemployment duration and unemployment incidence by reason regressions. He reports that the increase in average unemployment duration was produced by the joblessness experience of displaced workers. Duca and Campbell (2007) use U.S. annual data from 1960-2005 to apply simulation techniques. They attribute the rise in unemployment duration to both ageing of the population and a decline in job turnover, which might stem from decreased job security.

Three main economic explanations have been offered for the observed lengthening of the average duration of unemployment. In the first uptake, Baumol and Wolff (1998) construct a simple model and use time series data for the U.S. to conclude that institutional factors like changes in the unemployment insurance, in the rate of unionization, and in the minimum wage cannot account for the observed increase in unemployment duration. Instead they link average duration of unemployment to technical change, arguing that the acceleration of technical change has raised the share of the labor force that is unemployed in any period because plants close

[3]The relaxation of the Social Security Disability Insurance and Supplemental Security Income eligibility rules may also help to explain the increase in non-participation rates.

more often. Second, Mukoyama and Sahin (2009) evaluate a search model which they calibrate using CPS data covering the period 1971 and 2002. They note that increased within-group wage inequality, which translates into higher uncertainty about wage offer distribution, is likely to lead to longer periods of job search. Finally, Juhn et al. (2002) maintain that long-term changes in joblessness have been produced by adverse shifts in labor demand (lower long-term demand), reflected in low levels of hiring (Aaronson et al. (2010)).

In this chapter, we rely on censored quantile regression methods to analyze the changes in the U.S. unemployment duration distribution. Quantiles seem appropriate to analyze unemployment duration for two main reasons. First, the methodology estimates the whole quantile process of duration time conditional on the attributes of interest, which constitutes a complete characterization of the distribution of duration time. Quantiles provide a natural way of characterizing important concepts such as short- or long-term unemployment, by focusing on the relevant tails of the duration distribution. Second, from a methodological standpoint, it is worth observing that quantile regression provides a unified and flexible framework for such an analysis.

Changes over time in the distribution of unemployment duration may be framed as resulting from changes in the distribution of the conditioning variables such as the age distribution or from changes in the conditional distribution of duration itself. We use the Machado and Mata (2005) method to disentangle those effects. The basic building block is the estimation of the conditional distribution by quantile regressions; then, by resorting to resampling procedures, one estimates marginal distributions consistent with the estimated conditional model as well as with hypothesized distributions for the covariates. Comparing the marginal distributions implied by alternative distributions for the covariates one is then able to perform counterfactual exercises that isolate the different effects contributing to the overall change.

The data used in this inquiry are taken from the nationally representative Displaced Worker Surveys of 1988 and 2008. The DWS is a retrospective survey that has been conducted biennially since 1984. It contains information on the nature of the job lost and the subsequent joblessness duration of workers displaced by reason of plant closure, slack work, or abolition of shift or position. The DWS is particularly well suited to study the distributional shape of unemployment duration because, unlike the CPS, it is a representative sample of the flow of displaced workers and because it provides information on completed spells of unemployment.[4]

The chapter is organized as follows. Section 2 describes the data set, providing a careful comparison of the two Displaced Worker Surveys used. Section 3 outlines the econometric methodology. The basic regression results and the Machado and Mata decomposition results to sort out the forces behind the changes in unemployment duration are presented in Section 4 and 5. Section 6 deploys a mixture model to help interpret the results and section 7 includes a sensitivity analysis. Section 8 concludes.

3.2 Data

3.2.1 General Description

The data used in this inquiry are taken from the nationally representative, Displaced Worker Supplement to the February 1988 and 2008 Current Population Survey. The dataset - and changes in the survey, including the wording of the core displacement question and the recall period over which information on job loss is recorded - are well described elsewhere (see, for example Farber (1999)), so that only brief introductory remarks are required here. The DWS has been conducted biennially since 1984.

The survey asks individuals from the regular CPS if, in any of the three

[4]It is demonstrably harder to characterize the distribution of an unemployed population based on the stock rather than the flow of the unemployed persons (Lancaster (1990)).

years preceding the survey date, they had lost a job due to plant closing, an employer going out of business, a layoff from which the individual was not recalled, or other similar reasons. If the respondent has been displaced, he or she is asked a series of questions concerning the nature of the lost job and subsequent labor market experience, in particular, the time to find another job.

It contains information on the nature of the lost job and subsequent joblessness for workers displaced by reason of plant closure, slack work, or abolition of shift or position. Such data can be supplemented by extensive information on the personal characteristics of the worker contained in the parent CPS. The choice of the 1988 and 2008 surveys was guided by the need to use a comparable framework to the greatest extent. The 1988 DWS survey was the first to provide information for a single spell of joblessness (until 1986 the recorded jobless duration included multiple spells of joblessness). The 2008 survey is the most recent available survey with adequate data on joblessness duration. Still, there remain some issues of comparability that will be discussed below.

The DWS has a number of advantages over administrative data. First, unlike the unemployment registry, the DWS survey covers both recipients and non-recipients of unemployment benefits. Second, because it is retrospective, the information on unemployment duration is not censored at the time of the exhaustion of benefits. And, third, the DWS allows the identification of transitions of displaced workers to another job without any intervening spell of unemployment.

It is important to collect information on job-to-job transitions because a non-negligible portion of the displaced worker does not observe a joblessness experience. It is however worth noting that the incidence of this type of employment adjustment did not change from the 1988 survey to the 2008 survey.[5]

[5]From an analytical point of view, one is interested, of course, in all the routes taken by the workers following the occurrence of a displacement event. The consideration of direct job-to-job transitions creates, however, some ambiguity in the measurement of

There are inevitably some shortcomings of the DWS data. Retrospective data are subject to recall bias - individuals experiencing displacement in past years may be more likely to understate their jobless duration than are more recent job losers - and respondents are prone to round (to months and quarters) their reported spells of unemployment. Beginning with the 1994 survey, however, the period over which job loss is measured has been reduced from five to three years, which should reduce the recall bias problem.

As mentioned above, since the 1988 survey the measure of unemployment has referred to the length of the single spell of joblessness that followed the displacement event and resulted in reemployment. To be sure, the definition still does not require the unemployed individual to be engaged in active search, so that this single spell may include intervals of suspended job search/withdrawal, but it no longer includes multiple spells of joblessness. A more recent innovation which affects the 2008 survey is that the DWS unemployment data are no longer top coded (at 99 weeks of joblessness). An additional source of right censoring in the data stems from our inclusion (via the CPS) of those individuals who failed to find work after displacement but who were nevertheless economically active as of the survey date. Overall, the proportion of censored observations is, in our sample, around 17-19 percent.

Although we included those who wanted but never found employment after losing their jobs - as well as those individuals who transitioned directly into reemployment without any intervening spell of joblessness - we excluded individuals who were not economically active at the time of the survey. Further, because the nature of displacement is not well defined for certain individuals and sectors, those employed part time and in agriculture at the point of displacement were also excluded, as were those aged less than 20 years and above 61 years. These restrictions yielded

unemployment duration. We shall discuss below the practical consequences of using different measures (including or excluding job-to-job transitions) of unemployment duration.

a sample of 2,837 (63% from the original data) individuals for 1988 and 2,199 (70% from the original data) for 2008.

3.2.2 Comparability of the DWS Surveys

There are a number of comparability issues that need to tackled. First, and most importantly, whereas the 1988 survey is a five-year retrospective data set of displaced workers based on the question *'In the past five years, that is since January 1983, has ...lost or left a job because of a plant closing, an employer going out of business, a layoff from which...was not recalled, or other similar reason?"*, the 2008 survey is a three-year retrospective data set based on the question *"During the last three calendar years, that is, from January of 2005 through December of 2007, did (name/you) lose a job, or leave one because a plant or company closed or moved, (your/his/her) position or shift was abolished, insufficient work, or another similar reason?"*. If the response to the job loss core question was positive, the respondent was asked whether the reason for displacement was 1) plant closing, 2) slack work, 3) position shifted or abolished, 4) seasonal job ended, 5) self-employment failed, and 6) other reasons. In line with the CPS definition of job displacement, only the first three situations will be considered in this chapter.

Even though the slight change of wording is unlikely to raise any major comparison problems, the reduction of the retrospective period is potentially more serious. Since there is information on the year of displacement of the worker, one can minimize this problem excising from the 1988 sample the individuals displaced in 1983 and 1984.[6] But this procedure does not completely solve the issue. If an individual experienced multiple spells of joblessness (which affects a fraction of displaced workers) the interviewer has instructions to record the episode where the worker lost the job with the longest duration. It may well occur that after loosing

[6]Displacements that occurred during January of 1988 were also excluded. The 2008 survey does not include, by construction, workers displaced in 2008.

a long-tenure job during 1983 or 1984 an individual was displaced again during the 1985-1987 period. In this case, this displacement from a short-duration job is not registered. There is a clear implication for distortion of the distribution of job duration, with short job durations being likely to be under represented in the 1988 survey in comparison with the 2008 survey. But there is no unambiguous implication for the distribution of unemployment duration.[7]

Second, even though unemployment rates were falling and labor market conditions were improving over the survey periods, the cyclical conditions were not identical. In fact, the average state unemployment rate at the time of displacement is 2.4 percentage points lower in the 2008 survey than the 1988 survey. We expect that by conditioning the unemployment duration distribution on the unemployment rate, we will be able to isolate the impact of the business cycle.

Third, in both surveys the displaced workers are asked whether they received advance notice of impending their lay-off, but in the 2008 survey this question is restricted to written notice, where in the 1988 survey the individuals distinguish between informal and written notice. In order to make this variable as comparable as possible we will consider as notified only those workers who received written notice at least two months before the date of displacement.

Apart from these three comparability issues, which were considered in the analysis, we are convinced that the two DWS surveys provide an adequate framework for characterizing the evolution of the unemployment experience of displaced workers throughout the period 1985 up to 2007.

[7]Some checks can, however, be implemented. First, one can compare the job duration distribution for the 1983-1984 period with the 1985-1987 period. Second, one can exclude from both samples workers with fewer than two years of tenure in the pre-displacement job. And third, one can use our decomposition methodology to simulate the 2008 unemployment distribution with the 2008 job duration distribution. In all cases we arrive to the conclusion that the issue of multiple job spells does not significantly affect the comparison of the two unemployment duration distributions.

3.3 Econometric methodology

Comparing this counterfactual sample with samples of durations from the actual marginals for 2008 and 1988, it is possible to derive Oaxaca type decompositions for the entire distribution, rather than for just its mean. We use the methodology *M&M* proposed in Machado and Mata (2005) to decompose the observed changes in unemployment duration between 1988 and 2008, in those due to changes in the conditional distribution of durations (the β's) and those stemming from changes in the joint distribution of the covariates. Other decompositions of interest often involve isolating the contribution of a single covariate and a single coefficient.[8]

In this chapter we introduce a methodological innovation in terms of implementing censored quantile regressions to control for left censoring but also random right censoring. The censoring correction methodology is presented below.

3.3.1 Censored quantile regressions

Let T_i represent the duration of the "most representative" unemployment spell of individual i and z_i be the vector of covariates for the ith observation. We consider statistical models specifying , the pth ($p \in (0, 1)$) quantile of T as

$$Q_{y(T)}(p|z) = z'\beta(p) \tag{1}$$

where y is measured in log and $\beta(p)$ is a vector of QR parameters, varying from quantile to quantile.

Our sample provides information on complete unemployment durations, but there are some incomplete spells (right censoring). More-

[8]In the implementation of the method in this chapter we made the lower percentile equal to 0.10 and the upper percentile equal to 0.90. We estimated the quantile regression coefficients at equally spaced intervals of length 0.005. We then draw 1000 ($= m$) of such estimates with replacement. A code in STATA with the whole procedure is available on request.

over, to avoid problems with taking logs of very short spells (0 or close to 0 weeks) we, arbitrarily, censored durations inferior to 0.25 at 0.25 weeks. The sample information we consider may thus be represented by $(y_i^\star, z_i),\ i = 1, \ldots, n$ where $y_i^\star = \min[\max(y_i, l), u_i]$, u_i denotes the upper threshold for y_i and l the left censoring point ($l = \log(0.25)$). When observation i is not censored u_i was taken to be the potential censoring duration (for instance, for a spell of six weeks starting in March 2007, u_i was 44 weeks). The QR estimator minimizes the sample objective function

$$\sum_{i=1}^{n} \rho_p(y_i - \min[u_i, \max(z_i'b, l)])$$

with,

$$\rho_p(\epsilon) = \begin{cases} p\,\epsilon & \text{for } \epsilon \geq 0 \\ (p-1)\,\epsilon & \text{for } \epsilon < 0, \end{cases}$$

(Powell (1984, 1986)). Estimation was performed iteratively using Buchinsky (1994) ILPA procedure with the modification suggested by Fitzenberger (1997). The censored quantile algorithm is programmed in STATA as a do-file.[9] For the estimation of standard errors for the individual coefficients we resort to the bootstrap. Since the errors from the QR equation are not necessarily homogeneously distributed, to achieve robustness we resample (y, z, l, u) following the method of Bilias et al. (2000).

Due to censoring, it may not be possible to identify the whole quantile process. Let (p_l, p_u) represent the range of quantiles that can be consistently estimated. Technically, any p in that range must be such that

$$M_n(p) = E\{\frac{1}{n}\sum_{i=1}^{n} I(l + \xi < x_i'\beta(p) < u_i - \xi)x_i x_i'\}$$

is uniformly positive definite in n for some $\xi > 0$ (Fitzenberger (1997), Theorem 2.1).

[9]The algorithm is available upon request.

Finally, we apply the estimator defined in equation (1) in the M&M decomposition.

3.3.2 *M&M* decomposition method

Machado and Mata (2005) propose a generalization of the Blinder-Oaxaca (BO) decomposition to a quantile framework using Monte Carlo methods. The decomposition method starts from the distribution of individuals' characteristics and the estimation of quantile coefficients to obtain the unconditional distribution of unemployment duration, which are then used to conduct counterfactual exercises.[10]

The *M&M* algorithm is as follows:

1. Generate p_i, $i \in \{1, \ldots, m\}$ from a standard uniform distribution;
2. Estimate the corresponding $\hat{\beta}^t(p_i)$, i.e. estimate the pth regression quantile of y on z_i;
3. Generate a random sample of size m from a given z; denoted z_i^*, $i \in \{1, \ldots, m\}$.
4. Obtain $\hat{Q}_{y(T)}(p|z^*) = z^{*'}\hat{\beta}(p)$, which is a random sample from the marginal distributions of durations times implied by the model postulated for the quantile process and by the assumed joint distribution of the covariates.

The *M&M* algorithm generates a sample from the unconditional distribution of y for periods 0 and 1. From the algorithm it is clear that the estimator is a function of both characteristics and parameters obtained by estimating quantile regressions. This yields its applicability to

[10] Decomposition methods suggest a way for performing the detailed decomposition for the structural effect that has clear drawbacks (Firpo et al, 2011). The detailed decomposition of the wage structure effect arbitrarily depends on the choice of the omitted group and therefore its interpretation is not clear. The omitted group is defined as a nonwhite, single female who did not receive any written notice and whose displacement is not due to firm closure.

perform a decomposition analysis of changes in unemployment duration distribution.

The traditional response to this problem is to restrict the comparisons to the means of the two distributions (the so called BO decomposition). If we model the conditional expectation of the variable of interest in period t as $E[y(t)|z] = z(t)\beta^t \;\; (t = 0, 1)$, the decomposition reads

$$E[y(1)] - E[y(0)] = \underbrace{\{E[z(1)] - E[z(0)]\}\beta^1}_{\text{covariates}} + \underbrace{E[z(0)][\beta^1 - \beta^0]}_{\text{coefficients}}.$$

That is, the change in the mean of y is decomposed in the contribution of the changes in the conditioning variables and the changes in the conditional mean function itself. It is clear, however, that looking just at means is overly restrictive as a method for analyzing cases such as unemployment duration inequality, where the critical indicators relate to spread and tail weight.

In *M&M*, Machado and Mata propose a method to decompose the changes in a given distribution(y) in two periods (indexed by 0 and 1) in several factors contributing to those changes: that is, an BO type decomposition for the entire distribution,

$$\text{distrib.}y(0) \to \text{distrib.}y(1) = \begin{cases} \text{distrib.}z(0) \to \text{distrib.}z(1) \\ \\ \text{cond. distrib.}y(0)|z \to \text{cond. distrib.}y(1)|z \end{cases}$$

In *M&M* we estimate the marginal distribution of y by combining the conditional distribution estimated by quantile regression with any hypothesized distribution for the covariates. Comparing the marginal distributions implied by different distributions for the covariates one is then able to perform counterfactual exercises.

We are interested in four types of counterfactual simulation exercises. First, we want to estimate the density function of unemployment duration in 2008, corresponding to the 1988 distribution of covariates. When z is an estimate of the actual distribution of the covariates in the population, the resulting sample of durations is drawn from the actual marginal distribution. In this case, z_i^* may be obtained by drawing with replacement from the rows of z, the regressors' data matrix. considering a regression using the distribution of the covariates in 2008 ($t = 1$), then the resulting durations will constitute a simulated sample from the marginal distribution of durations that would have prevailed in 1988 ($t = 0$) if all covariates had been distributed as in 2008 (assuming, of course, that the β vector was estimated with 1988 data and that workers had unemployment durations according to 2008).[11] To compute this decomposition just follow the algorithm above but drawing the bootstrap sample of the third step from the rows of $z(0)$ instead of $z(1)$.

In the second decomposition exercise, we estimate the density function of unemployment duration in 2008, corresponding to the 1988 coefficients. This way it is possible to decompose the observed changes in those due to changes in the conditional distribution of durations (the β's). To do this decomposition exercise just follow the algorithm above for t equal to 1 but estimate the corresponding $\hat{\beta}^t(p_i)$ for t equal to 0.

The third and fourth decomposition exercises involve isolating the contribution of a single covariate or a single coefficient. Thus, we substitute a single covariate $z_j(0)$ or a single coefficient β in step 2 of the previous

[11] The samples generated in the counterfactual exercises are not a true sample of the distribution since they are based on estimates rather than on true parameters of the distribution. For the counterfactual exercise unconditional quantiles are required, that is, a set of unemployment duration values that are not dependent on the characteristics but that completely characterize the distribution of unemployment duration. Therefore, we need to estimate the marginal density function of unemployment durations. The difficulty lies in estimating a marginal density that is consistent with the conditional distribution defined in equation (1). However, Machado and Mata (2005) and Albrecht et al. (2009) show that when the size m of the sample is large enough the $M\&M$ procedure yields consistent and asymptotically normal estimators of the counterfactual distribution it is design to simulate.

algorithm.

Finally, by computing four differences between the conditional distribution in 2008 and the four counterfactual simulation exercises we obtain all the possible decomposition combinations that we will discuss in section 3.5.

3.3.3 Relevance of methodology

In this section we explore the potential of the methodology as a tool for analyzing duration data. We start by explaining the relevance of using quantile regression models instead of the traditional models specified in terms of hazard functions. We show the relevance of using $M\&M$ quantile decomposition method, instead of either the Blinder-Oaxaca decomposition or other known decomposition methods and finally, we present an overview of recent studies that have been using quantile decomposition method.

Hazard models are the most popular frame for duration analysis and therefore it is important to relate them with models for conditional quantile functions. Both constitute a complete characterization of the conditional distribution of duration, employment and unemployment in particular.

Quantile regressions are a natural and flexible alternative for the analysis of duration data in general and unemployment duration in particular. The analysis of various moments on the unemployment duration distribution affords important insights into the different determinants of short or long term unemployment duration. Distinguishing between short and long duration is particularly important in countries with tight employment protection legislation. In these countries, if a worker faces the prospect of losing her job this will mean that it will be difficult for her to find another job and therefore this will induce the individual to suffer a long unemployment spell. Quantile regressions can be specified according to (1). A typical example of an hazard function is the Accelerated Failure-Time (AFT) model which implies that,

$$Q_{logT}(p|x) = x'\beta + \sigma Q_{\epsilon}(p). \tag{2}$$

From the comparison of these two equations (1 and 2), the advantages of QR models over conventional proportional hazard models are clear.[12] Quantile regressions provide a more complete characterization of all the duration distribution. This methodology allows for the whole distribution not only the mean but also the median, skewness, tail behavior and all the distribution points to depend on the covariates.[13]. The conditional quantiles allow sufficient flexibility for some regressors to have a proportional impact, while others depict effects that are duration dependent. The QR model allows for the covariates to have different effects at different regions of the distribution. And we know that there are variables such as industry, that exert a statistically significant influence throughout the entire distribution. On the other hand, covariates such as age, education and firm size are more relevant in the tails.

Quantile regressions when compared to more traditional hazard rate models have three disadvantages (Fitzenberger and Wilke (2005)). First, quantile regressions involve only time-invariant covariates. Second, traditional hazard rate models account for competing risks in a direct way. Third, the method does not account for unobserved heterogeneity.[14]

Machado and Portugal (2002) show that quantile regressions can overcome some forms of neglected heterogeneity and baseline misspecification

[12] For an explicit and complete characterization of the link between quantile and hazard functions, see Machado and Portugal (2002) and Fitzenberger and Wilke (2005).

[13] There are hazard models in which the *beta* varies over of the support of the outcome variables (See Donald et al. (2000)).

[14] The presence of unobserved heterogeneity correlated with the covariates is a serious problem in regression analysis. Furthermore, even when the error term is uncorrelated with the covariates it is well known that unobserved heterogeneity biases the results towards negative duration dependence. In the present analysis we shall assume, as is conventional, that the regressors are uncorrelated with the error term. Also, our decomposition is not affected by the role of duration dependence.

in proportional hazard models. In addition, quantile regression analysis offers further advantages for studying duration data. First, the censored quantile regression estimator enables the accommodation of incomplete duration data. Second, quantile regression are analytically connected to hazard models but they have the advantage of imposing fewer distributional assumptions.

From a methodological vantage point, Machado and Portugal (2002) reveal that the conditional quantiles encompass the proportional hazard models, as they allow sufficient flexibility for some regressors to have a proportional impact, while others depict effects that are duration dependent.

And why should one be interested in using $M\&M$ decomposition method instead of other decomposition methods? In labor economics, but also in many other fields, many researchers are interested in understanding causal relationships. This is a very useful instrument to understand policy implications. On one hand, there are the composition effects related with changes in the attributes of the population. On the other hand, there are the structural effects related with changes in the determinants of unemployment duration. This means that it is important to understand how the increase in the proportion of older workers influenced the unemployment duration but it is also interesting from a policy point of view, to understand if older individuals are differently retained by firms. These decomposition methods allow to disentangle effects between composition and structural effects. This kind of exercise is made possible by estimating effects for each subpopulation and then run counterfactual exercises by simulating the effects by substituting the covariates and the coefficients for each subpopulation.

The most simple example is the Blinder-Oaxaca decomposition method, which is suitable to compute this kind of decompositions when one is interested in the mean of the variable of interest. After 2000 the most important development in decomposition methods has been the extended use of decomposition methods beyond the mean to other distributional parameters (Fortin et al. (2011)). Such an example of semi-parametric decomposition method is the one proposed in Machado and Mata (2005). In a comprehensive overview of decomposition methods that have been developed since the Blinder-Oaxaca decomposition, Fortin et al. (2011) find that decomposition have helped uncover the most important factors behind changes in the distribution of wages. This characteristic of decomposition methods has motivated a large number of studies since Blinder-Oaxaca was proposed in order to try to provide formal economic explanations for those distributional changes. As Albrecht et al. (2009) mention in their paper, "several recent papers use the quantile regression decomposition method proposed in Machado and Mata (2005)". Although $M\&M$ is computationally demanding, the success of the method has been attributed to the fact of providing a natural and data undemanding way of performing a detailed decomposition of the distribution of the variable of interest.

3.4 Composition and Structure

The basic pieces of information to our counterfactual analysis are the changes in the attributes (covariates) of the jobless population and the changes in the distribution of duration for any given level of those attributes ("structure" or coefficient changes). The latter are estimated by censored quantile log-linear regressions (Koenker and Bassett (1978) and Powell (1984, 1986)).

3.4.1 Covariates

Descriptive information on the two samples is provided in Table 3.1 and Figure 3.2. The composition of the 2008 sample differs from that of 1988 in some important ways.

Table 3.1: Sample descriptive statistics

	1985-1987	2005-2007
Age	36	41
Male	0.65	0.60
White	0.86	0.82
Married	0.60	0.52
Married female	0.17	0.18
Schooling (years)	12.5	13.2
Tenure (years)	4.54	4.81
Close	0.46	0.35
Written notice	0.05	0.10
State unemployment rate	7.06	4.67
Unemployment insurance	0.61	0.46
Proportion censored	0.17	0.19
Proportion duration is zero	0.12	0.12
Unemployment Duration (median in weeks)	8	8
Unemployment Duration / UR (Quantil 25)	0.28	0.43
Unemployment Duration / UR (Quantil 50)	1.13	1.28
Unemployment Duration / UR (Quantil 75)	2.83	3.43
Total number observations	2837	2199

Note: UR represents the state unemployment rate.

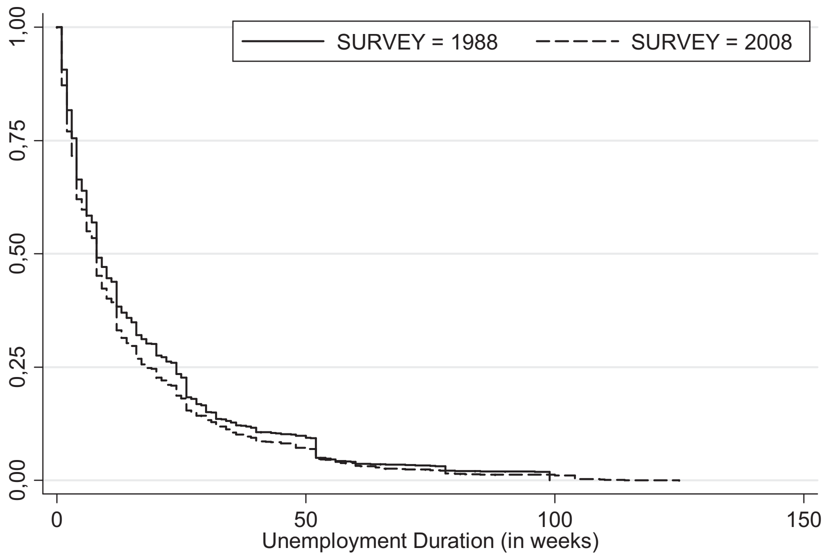
The median unemployment duration is stable between the 1985-87 period and the 2005-07 period. This indication is best understood in the empirical survival functions (Kaplan-Meier estimates) exhibited in Figure 3.2. Although this leftward shift is noticeable at both tails of the joblessness distribution, upper quantiles increased relative to the mean unemployment rate, as pointed out by Abraham and Shimer (2001) (Table 3.1). This indication is stronger if one considers the conventional measure of unemployment duration, where direct transitions without an intervening unemployment spell are excluded.

The proportion of direct job-to-job transitions (joblessness spells with duration equal to 0) did not change. In both periods these individuals involved in job-to-job transitions were not significantly different from the

Table 3.2: Sample descriptive statistics of direct job-to-job transitions

	1985-1987	2005-2007
Age	36	40
Male	0.65	0.62
White	0.91	0.93
Married	0.65	0.57
Married female	0.18	0.17
Schooling (years)	13.2	13.8
Tenure (years)	5.4	4.7
Close	0.63	0.44
Written notice	0.09	0.22
Unemployment insurance	0.10	0.07
Number observations	341	255

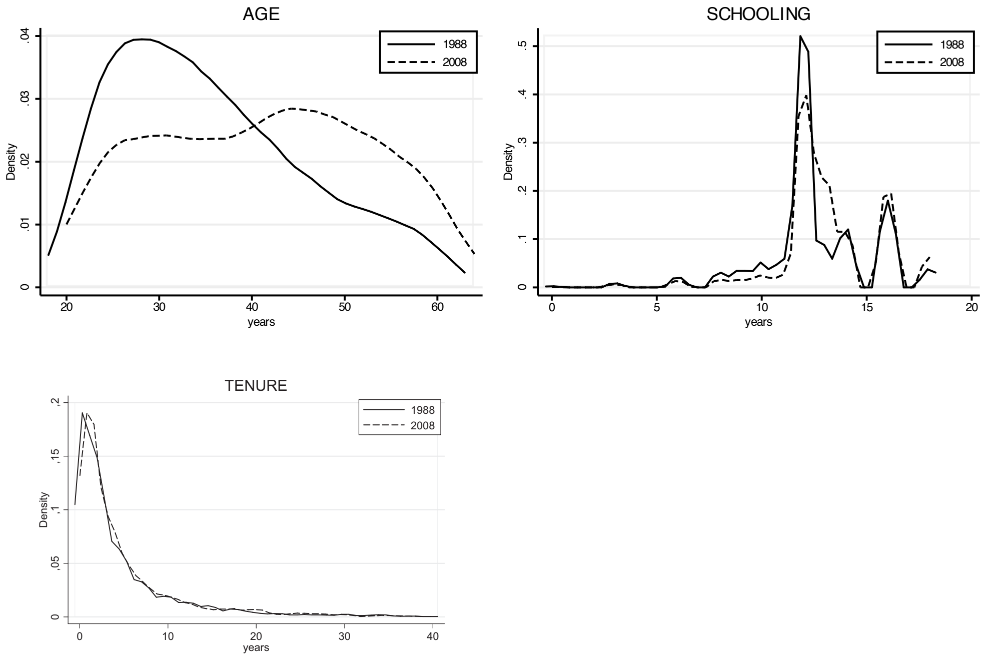
Figure 3.2: Kaplan-Meier survival functions



rest of the displaced group (see Tables 3.1 and 3.2).

Displaced workers in the latter survey are older and better educated

Figure 3.3: Kernel densities for age, schooling and tenure



than during the eighties, reflecting the aging of the baby-boom generation (see Figure 3.3). The proportion of female workers among displaced also increased sizably, probably because labor market participation rates of women increased and so did the risk of being displaced over the relevant period.

The likelihood of receiving formal notice of job lay-off more than doubled in the 2008 survey, probably due to the introduction of the Worker Adjustment and Retraining Notification Act, which was enacted in 1988, which made pre-notification of displacements mandatory for mass-layoffs or shut-downs generated by large firms (Addison and Blackburn (1994)).

Interestingly, despite the change in the reference period of job displacements (from five to three years), there are no significant changes in the distribution of job duration in the pre-displacement job (see Figure 3.3). It may still happen, however, that workers that are now displaced have longer tenure than before.

In a nutshell, displaced workers are older, more educated and experienced and more likely to be female than before.

3.4.2 Coefficients

We characterize the conditional distributions of jobless duration by quantile regression (QR) models.

Empirical results for selected quantiles from fitting the QR model are given in Tables (3.11) and (3.12) for both surveys. Focusing on the 1985-1987 survey, the regression coefficient estimates are fairly conventional.

Tenure coefficient is to be interpreted as the effect of tenure on unemployment duration over and above that resulting from age. It is known as a stylized fact that with the increase in age, as individuals get older, an unemployed individual will get less job offers (Gielen and van Ours (2006)). Age reduces escape rates and therefore it originates an increase in unemployment duration, proxying the reduced arrival rate of job offers with age. On the other hand, individuals with more tenure on the last job, invested more in specific firm human capital and presumably they had higher wages. Therefore, once they lost their job they will have more difficulty to adjust their wage to a lower level (higher reservation wages) (Addison and Portugal (1989) and Kletzer (1989)). And this is the reason why they will stay more time unemployed. The impact of tenure is statistically significant only at high quantiles. The result for race is familiar and captures the poorer opportunities facing non-whites as a result of both objective and discriminatory factors.

The familiar (opposing) effects of marital status on reemployment probabilities - positive for males and negative for females - are also obtained. The result for married males presumably picks up a household head effect, and thus likely reflects the higher opportunity cost of unemployment for married males and their greater search intensity.

It is worth noting that the variables that have significantly higher effects during the early phase of the unemployment spell very likely reflect the influence of on-the-job search (advance notice of displacement and dislocation by plant closing) or human capital (as captured by schooling). In the latter case it can be argued that larger human capital endowments are associated with greater job opportunities and higher opportunity costs of unemployment that necessarily erode with the progression of the unemployment spell. A number of explanations can be suggested here. Human capital depreciation, unobserved individual heterogeneity correlated with the measures of human capital, or stigmatization would lead to a fading human capital effect on the transition rate out of unemployment.

Schooling enhances the chances of getting a job, but much more so for low durations. It can be argued that larger human capital endowments are associated with greater job opportunities and higher opportunity costs of unemployment that necessarily erode with the progression of the unemployment spell. A number of explanations can be suggested here: human capital depreciation, unobserved individual heterogeneity correlated with the measures of human capital, or employers' stigmatization of long-term unemployed, would lead to a fading human capital effect on the transition rate out of unemployment. Like schooling, written pre-notification (defined as written notice of at least three months) and job loss by reason of plant closure have significantly higher effects during the early phase of the unemployment spell. This pattern reflects the influence of on-the-job search. Faced with the prospect of an imminent discharge, the worker will engage in on-the-job search. If successful, he or she will experience a short spell of unemployment (Addison and Portugal (1992)). Identically, workers displaced by reason of plant closing — in comparison with workers dismissed due to slack work or position shifted or abolished — benefit from an essentially short-term advantage conveyed by job search assistance and early (and unmistakable) warning of displacement (Gibbons and Katz (1991)).

Despite broad qualitative agreement between the regression coefficient

Table 3.3: Unemployment duration regression results for 1985-1987

	Q20		Q50		Q80	
Age	0.012	(0.006)	0.022	(0.005)*	0.014	(0.004)*
Male	0.031	(0.188)	0.335	(0.142)*	0.307	(0.116)*
White	-0.282	(0.176)	-0.269	(0.135)*	-0.377	(0.112)*
Married	-0.332	(0.155)*	-0.281	(0.119)*	-0.160	(0.098)
Married female	0.634	(0.249)*	0.818	(0.190)*	0.470	(0.154)*
Schooling	-0.122	(0.025)*	-0.050	(0.018)*	-0.047	(0.013)*
Tenure	-0.001	(0.011)	0.011	(0.008)	0.018	(0.006)*
Close	-0.730	(0.118)*	-0.433	(0.090)*	-0.182	(0.074)*
Written notice	-0.643	(0.262)*	0.234	(0.199)	-0.004	(0.161)
Constant	2.206	(0.418)*	2.065	(0.313)*	3.519	(0.253)*
Observations	2818		2674		2522	

Notes: The dependent variable, unemployment duration, is measured in logs. Quantile regression results based in 2837 observations.

estimates from the two surveys, there are, nevertheless, some differences. For their magnitude and potential impact on the unemployment duration distribution (see section 4.5.3), two are most striking.

First, the sharp decrease in the sensitivity of duration to schooling throughout the distribution. One may speculate that as displaced workers became more educated and experienced, the signaling power of schooling faded significantly. Second, the intercept also dropped sharply which reflects an overall shift to the left of the distribution of durations. The intercept will capture (among other things) all the time-varying common factors and, so, will certainly reflect the improved business cycle conditions in 2008.

Also worth noticing, but of limited quantitative impact, are the following findings. The jobless distribution became independent of gender: the market treats female and male displaced workers similarly (Abraham and Shimer (2001)). Although the being displaced by plant closing still significantly reduces the spell duration, the effect is much more attenuated in 2008.

Table 3.4: Unemployment duration regression results for 2005-07

	Q20		Q50		Q80	
Age	0.018	(0.005)*	0.023	(0.004)*	0.018	(0.004)*
Male	-0.043	(0.161)	-0.051	(0.116)	0.036	(0.134)
White	-0.538	(0.146)*	-0.243	(0.106)*	-0.433	(0.122)*
Married	-0.135	(0.147)	-0.231	(0.106)*	-0.269	(0.122)*
Married female	0.351	(0.225)	0.208	(0.163)	0.292	(0.188)
Schooling	-0.048	(0.024)*	-0.015	(0.017)	-0.028	(0.018)
Tenure	0.004	(0.010)	0.030	(0.007)*	0.033	(0.008)*
Close	-0.425	(0.118)*	-0.283	(0.084)*	-0.176	(0.096)
Written notice	-1.203	(0.186)*	-0.204	(0.127)	-0.049	(0.142)
Constant	0.844	(0.410)*	1.437	(0.288)*	3.030	(0.328)*
Observations	2159		2102		1943	

Notes: The dependent variable, unemployment duration, is measured in logs. Quantile regression results based in 2199 observations.

3.5 Changes in the unemployment duration distribution

Changes over time in the distribution of unemployment duration may result from changes in the distribution of the conditioning variables (e.g., labor force characteristics such as the age distribution) or from changes in the conditional distribution of duration itself (which may be thought of as changes in the way those labor force characteristics impact duration, the "coefficients"). The first is a composition effect and the second may be thought of as a "structural effect" (as in Autor et al. (2008)). Machado and Mata (2005) proposed a method (hereafter, *M&M* decomposition) for disentangling those effects. The method resorts to resampling procedures and it is based on the estimation of marginal distribution of the variable of interest consistent with a conditional distribution estimated by quantile regression, as well as with any hypothesized distribution for the covariates. Comparing the marginal distributions implied by different distributions for the covariates one will then able to perform counterfactual exercises and identify the sources of the changes in the distribution of duration over the twenty-year period (see chapter 3 for further details).

The results are presented in Table 3.5. The first two columns of this table refer to the marginal wage distributions in 1988 and 2008, while the third column presents the estimates of the overall changes which occurred during the period. The next three columns decompose total changes into changes due to the covariates (the 2008 estimated marginal versus the counterfactual 2008 marginal density if all attributes were distributed as in 1988); changes due to changes in the coefficients (the counterfactual 2008 marginal density if all attributes were distributed as in 1988); and residual changes, that is, changes unaccounted for by the estimation method.

Between the 1988 and 2008 survey, the distribution of unemployment duration shifted to the left, most notably at higher percentiles. Whereas unemployment duration decreased by 1.2 weeks at the median it decreased by 4.2 weeks at the 8th decile (see the third column of table 3.5). It is clear from columns 4th and 5th that (aggregate) changes in the coefficients were more influential driving the overall displacement of the unemployment duration distribution than (aggregate) changes in the covariates. "Coefficient changes" are everywhere larger, in absolute magnitude, than "covariates changes". Interestingly, whereas "coefficient changes" led to shorter durations above the median duration, "covariates changes" generated longer durations at the highest percentiles. At the 8th decile, unemployment duration increased by 3.1 weeks due to changes in covariates but decreased by 7.3 weeks due to changes in the coefficients.

Table 3.5: Contributions to changes in the quantiles of the unemployment distribution (weeks)

	Marginals			Aggregate contributions	
	1988	2008	Change	Covariates	Coefficients
10 th quant.	0.293	0.251	-0.042	0.001	-0.043
	0.266;0.320*	0.214;0.289*	-0.087;0.0030	-0.038;0.0410	-0.182;-0.107*
20 th quant.	1.460	1.186	-0.274	0.126	-0.399
	1.405;1.514*	1.141;1.231*	-0.342;-0.205*	0.068;0.183*	-0.542;-0.488*
30 th quant.	2.945	2.494	-0.451	0.353	-0.805
	2.881;3.010*	2.428;2.560*	-0.542;-0.360*	0.266;0.441*	-0.978;-0.919*
40 th quant.	4.969	4.215	-0.753	0.646	-1.399
	4.854;5.083*	4.122;4.309*	-0.897;-0.609*	0.521;0.771*	-1.690;-1.576*
50 th quant.	7.727	6.523	-1.204	1.031	-2.235
	7.574;7.880*	6.400;6.645*	-1.388;-1.020*	0.855;1.206*	-2.564;-2.412*
60 th quant.	11.613	9.796	-1.817	1.571	-3.388
	11.392;11.834*	9.628;9.964*	-2.077;-1.558*	1.342;1.800*	-3.713;-3.456*
70 th quant.	17.519	14.433	-3.086	2.320	-5.406
	17.188;17.849*	14.153;14.714*	-3.487;-2.684*	1.967;2.674*	-5.602;-5.282*
80 th quant.	26.392	22.197	-4.195	3.137	-7.332
	26.039;26.745*	21.796;22.597*	-4.720;-3.670*	2.628;3.646*	-6.978;-6.594*
90 th quant.	41.963	37.981	-3.983	3.815	-7.798
	41.333;42.594*	37.291;38.670*	-4.980;-2.985*	2.813;4.818*	-8.498;-7.752*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (2008"minus" 1988) of the marginal and of the counterfactual distributions (based on 1000 replications).

3.5.1 Composition Effects

As hinted above, the composition of the displaced workers (and the underlying economic environment) changed significantly between surveys: displaced workers became older and more educated; the proportion of females increased; written pre-notification of impending lay-off became more common; and the macroeconomic conditions improved. Overall, these changes produced longer jobless durations for all percentiles. A finer analysis, one that would enable us pinpoint the most influential regressors, requires the estimation of the impact of each of those changes on the conditional distribution of durations.

Using the techniques described in the methodology it is possible to isolate the contribution of the changes in the distribution of each covariate to the changes in the distribution of durations of joblessness spells. As it turns out, solely one explanatory variable exhibit a statistically significant composition effect: age (see Table 3.6). The results displayed in table 3.6 are obtained from the difference between predicted duration under 2008 covariates and coefficients and predicted duration for 2008 covariates and 2008 coefficients, except the covariate under examination which will take its 1988 values.

The ageing of the population translated into longer durations most notably, for the long-term unemployed (that is, those in the right tail of the unemployment duration distribution). Here, we estimate that at the 90th quantile duration is 2.5 weeks (11%) longer in 2008 than it would have been if the age of the population had been distributed as in 1988. As for gender, in particular, no significant impact is detected. The larger share of women in the population of displaced workers did not affect the shape of the unemployment distribution.

Table 3.6: Contribution of selected covariates to the change in the quantiles of the unemployment distribution

	Age	Male	White	Married	Married female	Schooling	Tenure	Close	Written notice
20 th quant.	0.10	0.00	0.03	0.05	0.01	-0.03	0.01	0.07	-0.08
	0.087;0.121*	-0.006;0.007	0.017;0.046*	0.036;0.061*	-0.001;0.025	-0.040;-0.015*	0.003;0.022*	0.053;0.089*	-0.098;-0.063*
50 th quant.	0.68	-0.02	0.05	0.14	-0.06	-0.04	-0.02	0.22	-0.09
	0.612;0.742*	-0.039;-0.002*	0.012;0.081*	0.098;0.192*	-0.094;-0.021*	-0.060;-0.020*	-0.066;0.033	0.170;0.269*	-0.115;-0.066*
80 th quant.	2.49	0.03	0.35	0.43	0.08	-0.27	0.31	0.41	-0.19
	2.283;2.695*	-0.048;0.111	0.192;0.515*	0.254;0.604*	-0.059;0.215	-0.382;-0.162*	0.110;0.503*	0.264;0.564*	-0.265;-0.109*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (2008“minus” 1988) of the marginal and of the counterfactual distributions (based on 1000 replications).

Table 3.7: Impact on duration (in weeks) of changes in quantile regression coefficients

	Constant	Age	Male	White	Married	Married female	Schooling	Tenure	Close	Written notice
20 th quant.	-4.03	-0.12	-0.01	-0.27	0.09	-0.07	0.59	0.06	0.15	-0.09
	-4.075;-3.994*	-0.144;-0.093*	-0.020;-0.003*	-0.294;-0.240*	0.071;0.100*	-0.085;-0.057*	0.564;0.612*	0.051;0.070*	0.135;0.165*	-0.101;-0.073*
50 th quant.	-8.24	0.50	-1.41	-0.10	0.08	-0.56	2.73	0.50	0.23	-0.19
	-8.347;-8.142*	0.466;0.528*	-1.466;-1.349*	-0.124;-0.079*	0.054;0.110*	-0.608;-0.515*	2.660;2.792*	0.462;0.534*	0.203;0.262*	-0.219;-0.158*
80 th quant.	-13.14	1.97	-3.52	0.17	-0.83	-1.29	6.44	1.67	0.06	-0.32
	-13.384;-12.891*	1.834;2.112*	-3.697;-3.343*	0.074;0.259*	-0.935;-0.718*	-1.430;-1.144*	6.299;6.588*	1.541;1.805*	-0.002;0.124	-0.401;-0.238*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (2008“minus” 1988) of the marginal and of the counterfactual distributions (based on 1000 replications).

3.5.2 Changes in the conditional duration

The major changes in the conditional distribution were a fall in the sensitivity of duration to the level of schooling of the displaced workers, the attenuation of the gender effect, and a sharp downturn in the intercept, which affects the white married males, with average schooling and tenure, displaced without notice and by reasons other than plant closing (see table 3.7). The values exhibited in table 3.7 are computed as the difference between estimated duration for the 2008 population and all 2008 coefficients, except the coefficient under scrutiny, which will take its 1988 value.

A one point percent increase in the male population generates a much larger unemployment duration decrease in the 2008 survey than in the 1988 survey. Indeed, if the male population regression coefficient of 1988 prevailed, the median unemployment duration would be 1.4 shorter (28%).

The increase in the tenure and the age coefficients implied an increase in median duration of 1 week and an increase at the 8th decile of 3.6 weeks (16%). It appears that, in the most recent displacement survey, being older translates into a even slower transition into employment than it was the case in 1988.

The fall in the schooling coefficient implied an increase in median duration of 2.7 weeks (54%). It appears that, in the most recent displacement survey, being more educated no longer translate into a faster transition into employment as it was the case in 1988. With some trepidation, we offer the tentative explanation that schooling is relatively more helpful in high unemployment than in low unemployment environments. It can be argued that under low unemployment regimes there is less heterogeneity among unemployed individuals (a higher proportion of truly unemployable workers), which will mean longer durations for a given (lower)unemployment rate (As predicted in our simple statistical model, below).

Using an argument similar to Blanchard and Diamond (1994) (footnote 6, page 423) being more educated is a weaker correlate of good quality when the unemployment is low.

3.6 Interpreting the results in a simple mixture model

Suppose that job-offers arise as a Poisson process with rate λ, and that there are two types of workers, A and B, with

$$\lambda_A > \lambda_B$$

The proportion of types A in the unemployment stock at t is denoted by $q(t)$. If all job offers are taken, the unemployment duration survivor function at t is

$$S(t) = q(t)\exp\{-\lambda_A t\} + (1 - q(t))\exp\{-\lambda_B t\}$$

It may be instructive to learn how in such a simple model one can generate the global patterns highlighted by the empirical analysis. Our empirical model identified two chief culprits:

- A composition effect: the ageing of the jobless population;
- A structural effect: the reduced sensitivity of unemployment duration to schooling.

The ageing of the baby boomers may be captured by a decrease in q, the proportion of individuals with higher exit rates.

$$\frac{\partial(1 - S(t))}{\partial q(t)} = \exp\{-\lambda_B t\} - \exp\{-\lambda_A t\} > 0.$$

Therefore,

$$\frac{\partial Q(\tau)}{\partial q} < 0,$$

Where $Q(\tau)$ is the duration quantile function. That is, a decrease in q ("ageing baby boomers"?) would increase the quantiles and, in particular the median duration. How does this impact vary over the distribution?

$$\frac{\partial^2(1 - S(t))}{\partial t \partial q(t)} = f_B(t) - f_A(t)$$

where $f(t)$ denotes the (exponential) p.d.f. of the two subpopulations. Therefore, there is a value of t ($t^\star = \ln(\lambda_B/\lambda_A)/(\lambda_B - \lambda_A)$), such that,

$$\frac{\partial^2(1 - S(t))}{\partial t \partial q(t)} < 0, \text{ for } t < t^\star \text{ and } \frac{\partial^2(1 - S(t))}{\partial t \partial q(t)} > 0, \text{ for } t > t^\star.$$

The impact of changes in q on duration quantiles in thus predicted to be U-shaped. For $1/\lambda_B = 16$ weeks and $1/\lambda_A = 2$ weeks, $t^\star \approx 2$ weeks. So in the range that QR can estimate it is natural to find an increasing effect.

In this exceedingly simple framework, the structural shock identified by the empirical analysis must be modeled by a reduction in the arriving rates of job offers, namely of λ_A (identifying A as the group with more schooling). Blanchard and Diamond (1994) argue that the exit rate from unemployment would be a decreasing function of unemployment duration. According to their "ranking assumption" (firms prefer to hire individuals that are unemployed for the least time), it is natural to infer that unemployment duration erodes the role of schooling as a signal of (unobserved) worker quality. The impact of such a change is

$$dS(t) = -t[\theta(t)d\lambda_A + (1 - \theta(t))d\lambda_B]S(t)$$

where

$$\theta(t) = q(t)S_A(t)/S(t)$$

Thus, if $\lambda_A < 0$ and $d\lambda_B < 0$, $dS(t) > 0$, and, consequently, the duration quantile function will shift to the right.

3.7 Sensitivity analysis

In this chapter we characterized the evolution of the unemployment experience of displaced workers throughout the period 1985 up to 2007 considering the two DWS survey extremes of the period interval, 1988 and 2008. Nevertheless, the methodology requirement of choosing only two moments in time could be driving the results. Therefore, it is important to consider another DWS survey in order to investigate how sensitive the results are to the choice of 1988 and 2008.

In this section, we replicate the analysis of section 4.5 for the time period 1988-1998. We chose 1998 as a final year because it fulfills the following three attractive features. First, it allows for a period of analysis that is big enough to use the proposed methodology in order to find any effect. Second, it is exactly in the middle of the base period of analysis, allowing to detect any changes and related effects in the unemployment distribution excluding clearly any possible influence of the 2008 distribution. Finally, in 1998 the unemployment rate was falling as in 1988 and in contrast with the increasing unemployment rate in 2008 (Figure 3.1).

The results of the comparison are given in tables 3.8, 3.9 and 3.10. There is a broad agreement, at least in terms of sign and statistical significance of the regression coefficients, in particular if we take the main effects as comparators. Between the 1988 and 1998 survey, the distribution of unemployment duration shifted to the left, most notably at higher percentiles. Whereas unemployment duration decreased by 2.4 weeks at the median it decreased by 6.3 weeks at the 8th decile (see the third column of Table 3.8). It is clear from columns 4th and 5th that (aggregate) structural changes in the labor market played a pivotal role.

In table 3.9 we observe the individual role of the composition changes and we find smaller effects between 1988 and 1998. The results confirm that the composition effects, in particular those related to age, appear to have played an important role. There is a new result between 1988 and 1998 but it is small in magnitude. We find that composition effects related to schooling decreased unemployment duration by 1 week at the 8th decile.

In table 3.10 we observe the individual role of the structural changes and we confirm the presence of major forces reshaping the unemployment duration distribution over period 1988-1998. Thus, there is a higher effect of unemployment duration to the male population, a higher effect of the unemployment duration to age; and we confirm the fall in the effect of the unemployment duration to the schooling levels. Although the sign and statistical significance is similar to the one find in the period 1988 and 2008, the magnitude of the effect of schooling and gender was smaller and the age effect was larger up to 1998. Indeed, if the male population regression coefficient of 1988 prevailed in 1998, the median unemployment duration would be 0.8 shorter. The fall in the schooling coefficient implied an increase in median duration of 1.2 weeks instead of 2.7 weeks (Table 3.7). Age implied between 1988 and 1998 an increase in the unemployment duration by 5.7 weeks at the 8th decile while between 1988 and 2008 the increase was by 2 weeks at the same decile. This difference in terms of magnitude seems to indicate that the role of schooling and gender have demonstrated an increasing trend along the period while the effect of age is loosing significance.

The results using 1998 instead of 2008 are in line with the baseline results. There are small differences in terms of magnitude, but in general we conclude that our results do not seem to be sensitive to the choice of the period of analysis.

Table 3.8: Contributions to changes in the quantiles of the unemployment distribution (weeks)

	Marginals			Aggregate contributions	
	1988	1998	Change	Covariates	Coefficients
10 th quant.	1.273	1.094	-0.179	-0.116	-0.064
	0,001;0,001*	0,001;0,001*	0,000;0,000*	0,000;0,000*	0,000;0,000*
20 th quant.	2.248	1.971	-0.278	-0.027	-0.251
	0,002;0,002*	0,002;0,002*	0,000;0,000*	0,000;0,0000	0,000;0,000*
30 th quant.	3.342	2.846	-0.496	-0.013	-0.483
	0,003;0,003*	0,003;0,003*	-0,001;0,000*	0,000;0,0000	-0,001;0,000*
40 th quant.	5.036	3.785	-1.251	0.044	-1.296
	0,005;0,005*	0,004;0,004*	-0,001;-0,001*	0,000;0,0000	-0,001;-0,001*
50 th quant.	7.709	5.291	-2.417	0.169	-2.586
	0,008;0,008*	0,005;0,005*	-0,003;-0,002*	0,000;0,000*	-0,003;-0,003*
60 th quant.	11.687	8.036	-3.651	0.335	-3.986
	0,011;0,012*	0,008;0,008*	-0,004;-0,003*	0,000;0,001*	-0,004;-0,004*
70 th quant.	17.631	12.634	-4.997	0.621	-5.619
	0,017;0,018*	0,012;0,013*	-0,005;-0,005*	0,000;0,001*	-0,005;-0,005*
80 th quant.	26.275	20.000	-6.275	0.544	-6.819
	0,026;0,027*	0,020;0,020*	-0,007;-0,006*	0,000;0,001*	-0,006;-0,006*
90 th quant.	42.267	33.907	-8.360	0.546	-8.906
	0,042;0,043*	0,033;0,034*	-0,009;-0,007*	0,000;0,0010	-0,008;-0,008*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (1998“minus” 1988) of the marginal and of the counterfactual distributions (based on 1000 replications).

Table 3.9: Contribution of selected covariates to the change in the quantiles of the unemployment distribution

	Age	Male	White	Married	Married female	Schooling	Tenure	Close	Written notice
20 th quant.	0.04	0.03	-0.01	0.10	0.05	-0.05	0.00	-0.01	-0.03
	0.025;0.062*	0.007;0.043*	-0.025;0.0100	0.075;0.119*	0.033;0.064*	-0.066;-0.031*	-0.008;0.0160	-0.015;0.0020	-0.043;-0.018*
50 th quant.	0.20	-0.12	0.02	0.07	-0.01	-0.18	0.01	0.02	-0.09
	0.153;0.247*	-0.159;-0.079*	-0.013;0.0480	0.024;0.111*	-0.047;0.0290	-0.211;-0.151*	-0.023;0.0450	-0.010;0.0400	-0.113;-0.061*
80 th quant.	1.08	-0.87	0.02	-0.50	-0.22	-1.16	0.05	0.01	-0.10
	0.868;1.297*	-1.007;-0.728*	-0.080;0.1280	-0.663;-0.332*	-0.390;-0.055*	-1.267;-1.046*	-0.126;0.2200	-0.046;0.0650	-0.155;-0.041*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (1998 "minus" 1988) of the marginal and of the counterfactual distributions (based on 1000 replications).

Table 3.10: Impact on duration (in weeks) of changes in quantile regression coefficients

	Constant	Age	Male	White	Married	Married female	Schooling	Tenure	Close	Written notice
20 th quant.	-2.21	-0.07	-0.26	0.21	0.17	-0.06	1.09	0.08	0.48	-0.05
	-2.257;-2.156*	-0.080;-0.055*	-0.279;-0.232*	0.180;0.238*	0.153;0.193*	-0.071;-0.052*	1.060;1.121*	0.070;0.093*	0.459;0.502*	-0.060;-0.039*
50 th quant.	-9.27	0.36	-0.75	-0.42	-0.29	-0.57	1.18	0.31	0.46	-0.20
	-9.490;-9.046*	0.321;0.390*	-0.786;-0.707*	-0.445;-0.387*	-0.335;-0.237*	-0.616;-0.528*	1.138;1.212*	0.284;0.331*	0.421;0.491*	-0.229;-0.165*
80 th quant.	-27.19	5.67	-1.08	1.07	-1.65	-0.74	3.03	0.81	1.01	-0.13
	-27.569;-26.817*	5.525;5.817*	-1.197;-0.960*	0.976;1.166*	-1.774;-1.526*	-0.845;-0.639*	2.856;3.204*	0.723;0.891*	0.906;1.111*	-0.190;-0.076*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (1998 "minus" 1988) of the marginal and of the counterfactual distributions (based on 1000 replications).

3.8 Conclusions

The starting point of this chapter was the evidence that measured unemployment duration in the U.S. increased substantially relative to unemployment rates. Here, the decomposition method proposed by Machado and Mata (2005) was employed in order to disentangle the contribution of the changes generated by covariates distribution and the conditional distribution. The estimation indicates that structural changes in the labor market played a pivotal role.

Composition effects related to age (but not gender) played a significant role. But, apart from this rather mechanical impact, important structural changes, captured in the changes of the regression coefficients, were at play. We have identified a major force reshaping the unemployment duration distribution: the change in sensitivity to schooling, increased the median unemployment duration 2.7 weeks.

We argue that the signaling power of schooling during the recent low-unemployment environment faded significantly. When the unemployment rate is low, the information that is passed to the employer through the schooling signal does not promote more job offers to the more educated unemployed. The information that is passed to the employer through the schooling signal is blurred when the unemployment rate appears to be very low. This finding raises the importance of discussing the interest of providing vocational training in order to help these workers to find a job.

Formal written pre-notification off impending redundancy leads to more intensive on-the-job search. This raises the importance of information asymmetry regarding the displacement decision. Identically, workers displaced with formal advanced notice benefit from an essentially short-term advantage conveyed by job search assistance and early warning of displacement.

This study raises a number of future lines of research. An extensive analysis of the CPS microdata should enable us to better characterize the link between the business cycle and the compositional changes of the stock of the unemployed workers. Furthermore, the information contained in the CPS can be explored to study the impact of recent changes in the unemployment insurance rules and to uncover the role of home ownership/ mortgage on worker mobility decisions.

Finally, a note of caution is in order. These results rely solely on the joblessness experience of displaced workers and may not apply to other unemployment experiences, for example, the unemployment experience of job market incomers and re-entrants or job quitters.

3.9 Appendix

Appendix A - Description of variables

Unemployment duration: The period an individual was unemployed for workers displaced by reason of plant closure, slack work, or abolition of shift or position. Measured in weeks.

Age: It registers the age of the individual. We restricted our analysis to individuals with age between 19 and 62 years old.

Male: It is a dummy that assumes value 1 for male individuals.

White: It is a dummy that refers to the race of the individual and it assumes value 1 for individuals that are white.

Married: It is a dummy that refers to the marital status of the individual and it assumes value 1 for married individuals.

Married female: It is a dummy that refers to the marital status of the individual and it assumes value 1 for married female individuals.

Schooling: Registers the number of complete years of schooling.

Tenure: Self reported duration of last job spell before displacement. It is measured in years.

Close: Plant closing.

Written notice: It is a dummy that assumes value 1 if the individual was notified of future displacement. We consider as notified only those workers who received written notice at least two months before the date of displacement.

Appendix B - Hazard and quantile regression

A comparison across different model specifications, meaning quantile regression, Cox proportional hazard and accelerated failure time is given in Tables (3.11) and (3.12) for both surveys. According to the proportional hazard rate specification, the exponential hazard rate is given by $\lambda_i = e^{X_i\beta}$ and $\lambda_i = e^{-X_i\beta}$ in the accelerated failure time specification. It is important to notice three aspects before comparing the results from the three specifications. First, the change in signs. Second, the coefficients from the proportional hazard and the accelerated failure time specifications will have the same magnitude but with opposite signs. Third, the sign of the expected duration in the accelerated failure time specification ($1/\lambda = e^{X_i\beta}$) is equivalent to the quantile regression model.

There is a broad agreement, at least in terms of sign and statistical significance of the regression coefficients, especially if we take the highest quantiles as comparators. The main exception is the regressors related to marriage. Female married individuals have a smaller effect in the quantile regression models than in either Cox or AFT models. Age and tenure in the previous job reduce the escape rates out of unemployment. Being unskilled decreases the chances of getting a job. We find the conventional opposing effects of being married on the probability of finding a job - positive for males and negative for females. A non-white has longer unemployment duration, whereas an individual displaced due to firm closure has decreased duration. The effects of pre-notification are statistically not significant.

Although the estimates from the Cox and the AFT model are on average precise, in some cases they provide an oversimplified vision of the impact of the covariates on the exit from unemployment. For regressors such as age, male and tenure, the proportional hazard estimates provide a good approximation as the impact of those covariates is roughly duration

Table 3.11: Unemployment duration regression results for 1985-1987

	Quantile Regression						Cox		AFT	
	Q20		Q50		Q80		Cox		AFT	
Age	0.012	(0.006)	0.022	(0.005)*	0.014	(0.004)*	-0.010	(0.002)*	0.014	(0.003)*
Male	0.031	(0.188)	0.335	(0.142)*	0.307	(0.116)*	-0.197	(0.061)*	0.265	(0.084)*
White	-0.282	(0.176)	-0.269	(0.135)*	-0.377	(0.112)*	0.183	(0.056)*	-0.248	(0.078)*
Married	-0.332	(0.155)*	-0.281	(0.119)*	-0.160	(0.098)	0.158	(0.051)*	-0.227	(0.070)*
Married female	0.634	(0.249)*	0.818	(0.190)*	0.470	(0.154)*	-0.393	(0.081)*	0.558	(0.112)*
Schooling	-0.122	(0.025)*	-0.050	(0.018)*	-0.047	(0.013)*	0.030	(0.008)*	-0.045	(0.011)*
Tenure	-0.001	(0.011)	0.011	(0.008)	0.018	(0.006)*	-0.009	(0.004)*	0.012	(0.005)*
Close	-0.730	(0.118)*	-0.433	(0.090)*	-0.182	(0.074)*	0.102	(0.038)*	-0.188	(0.055)*
Written notice	-0.643	(0.262)*	0.234	(0.199)	-0.004	(0.161)	0.009	(0.085)	-0.033	(0.118)
Constant	2.206	(0.418)*	2.065	(0.313)*	3.519	(0.253)*			2.504	(0.187)*
Observations	2818		2674		2522		2837		2837	
Scale parameter									1.393	(0.027)*
Shape parameter									0.787	(0.060)*

Notes: The dependent variable, unemployment duration, is measured in logs. Regression results based in 2837 observations.

Table 3.12: Unemployment duration regression results for 2005-07

	Quantile Regression						Cox		AFT	
	Q20		Q50		Q80		Cox		AFT	
Age	0.018	(0.005)*	0.023	(0.004)*	0.018	(0.004)*	-0.010	(0.002)*	0.016	(0.003)*
Male	-0.043	(0.161)	-0.051	(0.116)	0.036	(0.134)	0.045	(0.061)*	-0.038	(0.084)*
White	-0.538	(0.146)*	-0.243	(0.106)*	-0.433	(0.122)*	0.192	(0.056)*	-0.304	(0.078)*
Married	-0.135	(0.147)	-0.231	(0.106)*	-0.269	(0.122)*	0.070	(0.051)*	-0.156	(0.070)*
Married female	0.351	(0.225)	0.208	(0.163)	0.292	(0.188)	-0.077	(0.081)*	0.172	(0.112)*
Schooling	-0.048	(0.024)*	-0.015	(0.017)	-0.028	(0.018)	0.015	(0.008)*	-0.021	(0.011)*
Tenure	0.004	(0.010)	0.030	(0.007)*	0.033	(0.008)*	-0.016	(0.004)*	0.024	(0.005)*
Close	-0.425	(0.118)*	-0.283	(0.084)*	-0.176	(0.096)	0.105	(0.038)*	-0.199	(0.055)*
Written notice	-1.203	(0.186)*	-0.204	(0.127)	-0.049	(0.142)	0.132	(0.085)	-0.226	(0.118)
Constant	0.844	(0.410)*	1.437	(0.288)*	3.030	(0.328)*			1.975	(0.187)*
Observations	2159		2102		1943		2199		2199	
Scale parameter									1.434	(0.027)*
Shape parameter									0.587	(0.060)*

Notes: The dependent variable, unemployment duration, is measured in logs. Regression results based in 2199 observations.

independent.

Some covariates, however, have impacts that are far from proportional. The impact of schooling, written notice and plant closure are clearly decreasing with unemployment duration. The longer the individual stays unemployed, the smaller the impact of these factors on the escape rate from unemployment. These effects would not be detected by conventional hazard rate models.

As mentioned in section 3.3, from a methodological point of view, these results reveal that the hazard ratios estimated from models for conditional quantiles encompass the proportional hazard models as they allow sufficient flexibility for some regressors to have a proportional impact, while others depict effects that are duration dependent.[15]

Appendix C - Sensitivity analysis regarding the state of the labor market

Unobserved ability of displaced workers may depend on the state of the labor market. In a boom the average ability might be lower than in a recession because in a boom only "bad" firms close while in a recession also "good" firms may close. In this sensitivity analysis two peaks are compared so that the distribution of unobserved ability is similar. I collected from DWS data from 1992 and 2012. We chose 1992 and 2012 because it fulfills the following two attractive features. First, in both years, the unemployment rate was in a high peak. Second, it allows for a period of analysis that is big enough to use the proposed methodology in order to find any effect.

The results of the comparison are given in tables 3.13, 3.14 and 3.15 in the Appendix. Between the 1992 and 2012 survey, the distribution

[15] There are hazard function models which can allow for different effects across different quantiles (See section 3.3 and Donald et al. (2000)).

of unemployment duration shifted to the right, most notably at higher percentiles. Whereas unemployment duration increased by 6.7 weeks at the median it increased by 26.7 weeks at the 8th decile (see the third column of Table 3.13). In accordance with the main results, it is clear from columns 4th and 5th that (aggregate) structural changes in the labor market continue to play a pivotal role.

Table 3.13: Contributions to changes in the quantiles of the unemployment distribution (weeks)

	Marginals			Aggregate contributions	
	1992	2012	Change	Covariates	Coefficients
10 th quant.	1.650	1.854	0.204	0.152	0.051
	1.619;1.681*	1.816;1.892*	0.155;0.253*	0.105;0.200*	0.027;0.075*
20 th quant.	2.761	3.244	0.483	0.323	0.160
	2.719;2.803*	3.175;3.312*	0.403;0.563*	0.236;0.410*	0.125;0.194*
30 th quant.	4.278	5.857	1.579	0.720	0.859
	4.202;4.354*	5.698;6.017*	1.407;1.752*	0.517;0.923*	0.786;0.933*
40 th quant.	6.861	10.394	3.533	1.426	2.107
	6.729;6.994*	10.149;10.639*	3.257;3.809*	1.096;1.757*	1.990;2.223*
50 th quant.	10.211	16.896	6.684	2.206	4.479
	10.025;10.398*	16.567;17.225*	6.308;7.061*	1.746;2.666*	4.306;4.651*
60 th quant.	14.729	25.953	11.225	2.996	8.229
	14.506;14.951*	25.487;26.420*	10.708;11.741*	2.323;3.668*	7.955;8.503*
70 th quant.	20.834	38.548	17.714	3.585	14.129
	20.515;21.154*	37.934;39.163*	17.015;18.413*	2.726;4.444*	13.794;14.465*
80 th quant.	29.577	56.255	26.678	3.993	22.685
	29.203;29.952*	55.452;57.059*	25.793;27.563*	2.862;5.125*	22.190;23.180*
90 th quant.	44.757	85.347	40.590	4.361	36.229
	44.197;45.317*	84.338;86.356*	39.468;41.712*	2.982;5.741*	35.663;36.795*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (2012“minus” 1992) of the marginal and of the counterfactual distributions (based on 1000 replications).

Table 3.14: Contribution of selected covariates to the change in the quantiles of the unemployment distribution

	Age	Male	White	Married	Married female	Schooling	Tenure	Close	Written notice
20 th quant.	0.19	-0.03	0.04	0.13	0.00	-0.08	0.01	0.19	-0.06
	0.159;0.224*	-0.050;-0.009*	0.014;0.057*	0.099;0.165*	-0.025;0.0190	-0.113;-0.051*	-0.017;0.0370	0.163;0.225*	-0.081;-0.035*
50 th quant.	1.84	-0.14	0.15	0.30	-0.20	-0.88	0.05	0.38	-0.06
	1.665;2.017*	-0.214;-0.059*	0.061;0.248*	0.171;0.438*	-0.317;-0.080*	-1.011;-0.758*	-0.067;0.1710	0.285;0.473*	-0.103;-0.025*
80 th quant.	5.57	-0.20	0.18	0.77	-0.92	-2.91	0.61	0.09	0.06
	5.073;6.072*	-0.376;-0.033*	-0.044;0.4120	0.387;1.143*	-1.255;-0.581*	-3.293;-2.528*	0.322;0.898*	-0.046;0.2230	-0.029;0.1490

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (2012“minus” 1992) of the marginal and of the counterfactual distributions (based on 1000 replications).

Table 3.15: Impact on duration (in weeks) of changes in quantile regression coefficients

	Constant	Age	Male	White	Married	Married female	Schooling	Tenure	Close	Written notice
20 th quant.	0.22	-0.06	0.34	-0.02	-0.28	0.08	-0.39	0.08	0.25	-0.04
	0.187;0.245*	-0.096;-0.021*	0.317;0.359*	-0.034;-0.001*	-0.298;-0.263*	0.065;0.089*	-0.418;-0.369*	0.065;0.088*	0.227;0.276*	-0.052;-0.030*
50 th quant.	4.73	1.49	0.76	0.38	-0.83	0.47	-2.93	0.12	1.43	0.03
	4.380;5.072*	1.413;1.563*	0.709;0.813*	0.347;0.422*	-0.896;-0.773*	0.417;0.525*	-3.039;-2.821*	0.093;0.146*	1.349;1.516*	0.009;0.047*
80 th quant.	31.62	-0.09	0.19	1.00	-1.07	0.96	-6.69	-0.11	2.88	0.19
	31.189;32.047*	-0.207;0.0340	0.094;0.276*	0.895;1.098*	-1.186;-0.945*	0.828;1.096*	-6.915;-6.474*	-0.185;-0.034*	2.689;3.063*	0.123;0.247*

Notes: Median and 95% interval estimates (in weeks) of the changes in the quantiles (2012“minus” 1992) of the marginal and of the counterfactual distributions (based on 1000 replications).

In table 3.14 we observe the individual role of the composition changes and the results confirm that the composition effects, in particular those related to age, appear to have played an important role. This result between 1992 and 2012 confirms the results between 1988 and 1998. We find that composition effects related to schooling decreased unemployment duration by 3 week at the 8th decile.

The results using 1992 and 2012 are largely in accordance with those of the baseline. However, there are two differences that are worth noting. Between the 1992 and 2012 survey, the distribution of unemployment duration shifted to the right, most notably at higher percentiles. Whereas unemployment duration increased by 6.7 weeks at the median it increased by 26.7 weeks at the 8th decile (see the third column of Table 3.13). These results are broadly explainable by the particular nature of the crisis covered by the 2012 survey.

The second difference regards the individual role of the structural changes (Table 3.15). It was revealed that in a period where the unemployment rates are higher the sorting effect of education is stronger. The schooling coefficient implied between 1992 and 2012 a decrease in the unemployment duration by 6.7 weeks at the 8th decile while between 1988 and 2008 the increase was by 6.4 weeks at the same decile. When the unemployment rate is higher the unemployment duration is decreased for the more educated.

Chapter 4

Determinants of earnings losses of displaced workers

4.1 Introduction

Every year, thousands of workers all over the world are affected by the negative consequences of displacement (Kuhn (2002)). Worker displacement is the subject of an extensive and growing literature. The costs of job loss in terms of unemployment, future employment and earnings-change have been the most studied aspects of job displacement. Displaced workers are defined in this chapter as all workers who separate from a dying, or shrinking firm in a given year.

During the 1980s a number of empirical studies appeared analyzing workers' post-displacement wages in the U.S. [(see, for instance, Podgursky and Swaim (1987), Kruse (1988), Addison and Portugal (1989), Kletzer (1989)].[1] Basically, these studies provide a snapshot view of short-term earnings losses, defined as the difference between pre- and post-displacement earnings of displaced workers.

However, this type of analysis, focusing solely on workers who have

[1]See Hamermesh (1989) for an enlightening discussion of this literature.

been displaced, is likely to underestimate the magnitude of wage losses, since it does not account for the earnings growth that would have occurred in the absence of job loss. A simple comparison of pre- with post-separation earnings for displaced workers is insufficient. The seminal paper by Jacobson et al. (1993) introduced a different approach to the study of worker displacement and earnings losses. These authors compare the earnings changes of displaced workers over a long-term period with the earnings changes that would have occurred if the displaced had not lost their jobs. Since this latter outcome variable is not observable, a comparison group of non-displaced workers is used. The emphasis in worker displacement research has shifted from short-term wage losses to long-term dynamics. In fact, in recent years the existence of suitable longitudinal data sets in the U.S. and Europe matching workers and firms enable the comparison of wage patterns for displaced and identical non-displaced workers.

Few of these studies, however, attempted to appropriately decompose the earnings gap between displaced and non-displaced workers into its main determinants. Understanding the causes of these reductions might shed some light on potential policy options to ease the burden of adjustment on these workers.

Hence, the main goal in this study is to measure the earnings losses of displaced workers resulting from firm closure, collective or individual dismissals having in mind that as in Jacobson et al. (1993) joblessness and wage rate decline play an important role. Furthermore, monthly wage losses are decomposed into different components related to worker, firm, and job title characteristics (both observed and unobserved). Taking into account job characteristics is crucial to obtain reliable estimates of the earnings losses following displacement, since earlier empirical work has shown that industry, firm, and match characteristics are an important determinants of earnings (see, for example, Podgursky and Swaim (1987), Addison and Portugal (1989), Carrington (1993), Neal (1995)).

Joblessness can depreciate general, sector and firm specific human capital and, compared to a similar worker who was not displaced, it prevents the accumulation of human capital through on-the-job training. It is worth noting that human capital has a decisive role during the early phase of the unemployment spell. It can be argued that larger human capital endowments are associated with greater job opportunities and higher opportunity costs of unemployment that necessarily erode with the progression of the unemployment spell. A number of explanations can be suggested here. Human capital depreciation, unobserved individual heterogeneity correlated with the measures of human capital, or stigmatization by employers would lead to a fading human capital effect on the transition rate out of unemployment. Mroz and Savage (2006) find that young workers that experience involuntary unemployment invest more in the short run in their human capital in order to mitigate any adverse effects coming from unemployment. Nevertheless, they are not able to fully recover from these adverse effects.

Several papers have studied the sources of the growth of wages in light of the human capital theory (Dustmann and Meghir (2005), Gathmann and Schönberg (2010) and Amann and Klein (2012)). The contribution of human capital to the wage growth has been decomposed in several components, the contribution of the acquisition of general, firm and job (or task) -specific human capital. In this chapter we explore these components to desintangle the wage loss that workers experience after displacement.

Regarding the wage rate decline, from a theoretical point of view, it is to be expected that reemployment wages of displaced workers will be lower than those of workers who remain employed. As mentioned by Fallick (1996), there are at least four reasons that can explain this pattern. First is the loss of human capital specific to the firm or industry. To the extent that these skills are non-transferable, their contribution to worker's productivity is permanently lost when a job loss occurs. Second, payments by seniority in order to provide incentives not to shirk may delay higher earnings to the latter part of the career. In this case, a permanent separation reduces lifetime earnings. Third, there is the loss of a high quality job match between the worker and the firm.[2] In fact, some authors find that standard estimates of the return to job-specific training are biased upward by job match and individual unobserved heterogeneity.[3] A long job tenure may signal a high quality match between the firm and the worker and/or a high ability worker, because more able workers and workers in good jobs are less likely to separate. Along this line of research, Addison and Portugal (1989) and Kletzer (1989) showed that tenure in the pre-displacement job is positively associated with post-displacement earnings, reflecting heterogeneity in worker ability and the transferability of skills. Fourth, to the extent that the firm's and/or industry characteristics also play a role in the process of wage determination, a displaced worker may lose some wage premium that he was previously receiving, such as insider rents, union premiums, or efficiency wage differentials.

Thus, beyond worker and firm characteristics, a third important dimension of wage formation is considered in this study - job title heterogeneity. Job title heterogeneity may influence wage rates for a number of reasons. First, it is well known that tasks that involve risks of death or serious accident are better paid than less risky tasks. One should therefore expect significant compensating differentials for occupations such as deep sea divers or bullfighters. Second, jobs that need to be executed under difficult or stressful conditions are also expected to be better remunerated than jobs that take place under pleasant conditions. For example, one should observe higher wages for individuals working on offshore oil platforms or in mines. Third, the complexity of some tasks may require intense special training and/or unusual skills. This is the reason why, for example, brain surgeons or jet fighter pilots have higher earnings. Fourth,

[2]However, displacement might increase earnings. For instance, if displacement dissolves a bad job match which was not perceived as such by the employee.

[3]See, among others, the studies of Abraham and Farber (1987, 1988), Altonji and Shakotko (1987), Topel (1991) and Dustmann and Meghir (2005).

some occupations are known to be chronically overcrowded, whereas others are thought to be in excess demand. For instance, for decades it has been argued that there is an oversupply of teachers and an undersupply of nurses. Fifth, by their nature some jobs put the workers in a position where they can inflict serious losses on their employers and/or the society. In such cases, the trade unions are powerful enough to extract significant rents in the form of higher wages. Industrial action by commercial airline pilots, flight controllers, train motormen, or more generally, by workers that are part of the natural monopolies workforce, often leads to a substantial wage premium. Sixth, entry barriers to some occupations, such as those ruled by worker associations (for example, closed shop occupations, medical associations, lawyers' associations, etc.) also enhance the labor income of their members. Seventh, the kind of technology being used may favor the organization of labor through unionization of the workplace, allowing unions to push for higher wages. Production activities that imply the concentration of a large number of workers in a single plant (say, the auto industry or ship building) facilitate industrial action, and thus, better worker conditions.

Potential losses of displaced worker can be related to the firm and job-title that they held before and after displacement. The heterogeneity among firms wage policies is very large and accounts for more than one third of the wage total variation (Torres et al. (2012)). Different wage policies are favored by the existence of industry rents (due to unionization or incentive pay premiums) or the operation of wage efficiency policies. In such an environment, the worker may benefit from engaging in job search to locate the firms with more suitable (more generous) wage offers. Good matches will be made and survive. Bad matches will be resisted and undone. However, with the occurrence of a displacement event, successful job searchers may loose their "job shopping" investment.

The role of job-title heterogeneity explaining total variation is also significant (around 50 percent). Job-titles summarize the general and specific skills of the worker, in particular those that are industry and occupation specific. Given the way those job titles were identified, they may also reflect the bargaining power of the workers. Because job-titles contain the skill requirements of the position held by the worker, it will also retain the hierarchical standing of the workers. Again, with the event of a displacement, a human capital will be destroyed, largely associated with the loss of his pre-displacement job-title. This was previously measured by looking at the effect of industry and occupation mobility. Our study adds to the existing literature by addressing directly this source of wage loss by looking at job-title fixed effects.

To properly incorporate these plethora of wage determinants a wage equation with three high-dimensional fixed effects - worker, firm, and job title - will be estimated using a nationally representative matched employer-employee data set - *Quadros de Pessoal*. The universal coverage of the employed population in the private sector in Portugal combined with the appropriate tools creates the optimal conditions for this exercise.

Two main objectives drive the investigation. The first is to investigate the monthly earnings losses by following Jacobson et al. (1993) (JLS) methodology, including transitions to zeros whenever the individuals are out of work. The second objective is to extend the Jacobson et al. (1993) (JLS) methodology by incorporating firm and job title fixed effects in the monthly wage equation (excluding transitions to zeros), allowing us to estimate the monthly wage losses of displaced workers. We decompose the monthly wage losses into their main sources using the methodology developed in Gelbach (2010). Basically, we use the fixed effects to explain part of the wage loss.

The structure of the remainder of the chapter is as follows. In Section 2 a brief review of the literature is given. Section 3 summarizes the institutional wage setting in Portugal. Section 4 describes the data and the sample construction. The empirical strategy is presented in Section 5 and Section 6 reports the results. Section 7 concludes.

4.2 Earlier literature on earnings losses of displaced workers

As mentioned before, there is an extensive empirical literature on the earnings impact of worker displacement.[4] For a variety of surveys and methodologies the studies for the U.S. have established that displaced American workers usually experience short spells of unemployment, but substantial and persistent reductions in earnings - on the order of 8 to 25 percent for prime-aged workers, in comparison with their non-displaced counterparts (Couch and Placzek (2010)) lasting over 15-20 years (von Wachter (2010)). This literature also establishes two stylized facts - high-tenure workers and industry switchers suffer the greatest earnings losses (see, for example, Kletzer (1989), Jacobson et al. (1993), Carrington (1993), Neal (1995) and Stevens (1997)).

Ruhm (1991) and Stevens (1997) use the Panel Study of Income Dynamics (PSID) and find that in the 1970s and early 1980s, the post-displacement earnings of displaced workers dropped between 7 and 13 percent.

Jacobson et al. (1993) use administrative earnings records from the Unemployment Insurance (UI) system of Pennsylvania for the period 1974-86. Their sample includes workers aged between 20 and 49 who reported positive earnings in the first quarter of the sample's period, were continuously employed during the first six years in a firm with at least 50 employees, and reported positive earnings at least once thereafter. Workers are considered to be displaced whenever their firm faces a drop in employment of at least 30% in the year before displacement. They found that high-tenure displaced workers suffer long-term earnings losses averaging 25 percent per year six years after displacement. These losses start to appear approximately three years before separation and are substantial even for workers reemployed in similar firms.

[4]See Fallick (1996) and Kletzer (1998) for surveys.

Couch and Placzek (2010) have cast some doubts on the magnitude of the estimates obtained by Jacobson et al. (1993). They argue that the results should be interpreted with some caution, as in the late 1970s and early 1980s U.S. industry suffered a significant restructuring that had a considerable impact on the state of Pennsylvania in particular. Using data for the state of Connecticut for the 1993-2004 period, their estimates are roughly half those found for Pennsylvania. They also found that long-term earnings losses are greater among unemployment insurance (UI) recipients, which seems to explain the difference in the earnings losses estimates across the two samples, as the data for Pennsylvania report a high incidence of UI receipt when compared with the Connecticut data.

The studies by Schoeni and Dardia (2003) and von Wachter et al. (2009) for California, and by Kodrzycki (2007) for Massachusets based on data for the 1990s, show that the magnitude and persistence of the losses are fairly consistent across different states of the U.S.

Using data from the Displaced Workers Survey (DWS) for different time periods, Farber (1993, 1997, 2005) finds that displaced American workers lose around 8 to 12 percent in comparison with their non-displaced counterparts. Using also DWS data for workers who lost their jobs in the recent recession of 2007-2009, Farber (2011) reports an estimate of 11 percent, i.e., full-time job losers who find new full-time jobs earned 11 percent less, on average, at their new jobs than they would have had they not been displaced.

Regarding losses by worker characteristics such as gender, age, and education, some conclusions seem to emerge in the most recent studies for the U.S. Using data from the DWS for 1981 to 2003, Farber (2005) finds that while in the 1980s more educated displaced workers experienced smaller earnings decreases in comparison with their less-educated nondisplaced counterparts, the situation seems to be reversed in the early 2000s. Regarding gender or race, no significant differences were found. von Wachter et al. (2009) also found that in California, in the 1990s, workers with a college degree had smaller earnings losses than workers without a high school degree, who, on the other hand, performed better than workers with some college or a high-school degree.

Chan and Stevens (2001) use the Health and Retirement Surveys (HRS) and find that older displaced workers suffer greater losses in earnings than those found for prime-aged workers when using DWS or PSID data. Kletzer and Fairlie (2003) use the National Longitudinal Survey of Youths (NLSY) to analyze the earnings losses of young workers, aged between 14 to 36 years, in the period from 1979 to 1993. They find that younger workers have similar long-term earnings losses in comparison with individuals having greater labor market experience.

For Europe the empirical evidence is less clear-cut. Some studies have concluded for the existence of large earnings losses (Bender et al. (2002) and Lefranc (2003)), while others have concluded for the existence of reduced earnings losses (Burda and Mertens (2001), Lehmann et al. (2005) and Hijzen et al. (2010)). On one point, however, these studies seem to be in agreement. A displaced worker who experiences a period of non-employment suffers a large penalty in earnings (Gregory and Jukes (2001), Bender et al. (2002) and Abbring et al. (2002)).

Burda and Mertens (2001) provide estimates for Germany using data from both the German Socioeconomic Panel (GSOEP) and the Social Insurance File (IAB) covering the 1985-94 period. They found a modest wage decline upon reemployment (about 3.6% in the year following displacement). They also concluded that large wage losses are associated with changes of industry, but not of firm.

Couch (2001) also used the GSOEP from 1988 to 1996 in order to examine the effects of displacement due to plant closure on annual earnings and unemployment duration. He reported an estimated loss of around 13.5% in the displacement year and a loss of 6.5% two years later.

Lefranc (2003) analyzed the sources of wage losses of displaced workers in France and the U.S. using micro-data from labor force surveys. He showed that while the magnitude of the wage losses are very similar in the two countries (around 10 to 15 percent), the sources of wage adjustment differ considerably. In the U.S., earnings losses stem mostly from the loss of search rents on the displacement job, while in France, most of the earnings losses result from the loss of accumulated firm-specific human capital.

Using labor force survey data from Estonia covering the period from 1989 to 1999, Lehmann et al. (2005) find that the main cost of displacement is the cumulative income loss measured as the difference between wages and out-of-work benefits, which is large for the minority of workers who experience long-term non-employment.

Hijzen et al. (2010) used a matched employer-employee data set for the U.K. to estimate the income loss of displaced workers from firm closure and mass layoffs. They showed that workers whose firm closes down lose 18-35 percent per year of their income, while workers who exit a firm that suffers a mass layoff lose 14-25 percent. In contrast to JLS, they found that income losses are driven mainly by non-employment spells rather than by wage losses.

Dustmann and Meghir (2005) study the sources of wage growth of young German workers estimating the returns to experience, sector tenure and firm specific tenure, identifying respectively the contribution of general, sector and firm specific human capital. Displaced workers due to firm closure are used to control for selection due to unobserved heterogeneity. Better types of workers find a job quickly and therefore, there is a potential problem of endogeneity. Dustmann and Meghir (2005) use a control function as Heckman and Robb (1985) using age as an instrument in the entire sample of new jobs and including the residual terms for endogeneity and selection correction in the wage function. They find that unskilled workers benefit from being attached to a particular firm while skilled workers benefit from the acquisition of transferable skills.[5]

[5]Jacobson et al. (1993) explain on page 696 that the selectivity bias can be sub-

Amann and Klein (2012) analyze the contribution of the acquisition of general skills and firm-specific skills to the wage growth. The returns to firm tenure might be biased because tenure is endogenous to wages. Therefore, they also use a control function estimator but instead of age they use with-in job variation in tenure as the instrument. They extend the Altonji and Shakotko (1987) by adding the interaction term between the endogenous variable and the first-stage residual. This allows them to find that longer lasting matches are characterized by high wage growth in the first five years and higher wages on average.

Gathmann and Schönberg (2010) use data on tasks performed in occupations from Germany to analyze skill transfers between occupations. Using a measure of the skill distance between occupations they are able to summarize the similarity of occupations in terms of different tasks (analytical, manual and interactive tasks). They find that individuals move to occupations that are at a short distance in terms of skill requirements. This distance decreases with the actual worker experience. The task-specific human capital explains up to 52% of overall wage growth over the career. Wage losses of displaced workers will be 10 percentage points larger for workers reemployed in a very distant occupation, that is, as a bank or insurance clerk, than for workers who can find employment in an occupation with similar skill requirements, for example, as a warehouse keeper.

4.3 Wage setting in Portugal

Portugal is considered to have a regulated labor market, with minimum wages, strong employment protection, and collective bargaining widely applied (OECD (2001) and Cardoso (2006)). In the 1990s Portugal was characterized by low unemployment rates, approximately 3-4 percentage

stantially reduced by restricting the attention to workers who separate from firms that closed down.

points below the EU-15 average. In 1994, the minimum legal monthly wage was 246 euros, representing around 37% of the median total monthly earnings of full-time employees (Eurostat).[6]

The Portuguese Constitution provides the juridical principles of collective bargaining, and grants unions the right to negotiate. The effects of the agreements are formally recognized and considered valid sources of labor law.

Concerning the bargaining mechanisms, a distinction should be made between the conventional regime and the mandatory regime. Conventional bargaining results from direct negotiation between employers' and workers' representatives. A mandatory regime, on the other hand, does not result from direct bargaining between these two, but is instead dictated by the Ministry of Labor. The Ministry can extend an existing collective agreement to other workers initially not covered by it or it can create a new one if it is not viable to extend the application of an existing document. A mandatory regime is applied when workers are not covered by unions, when one of the parties involved refuses to negotiate, or bargaining is obstructed in any other way.

Beyond the existence of compulsive extension mechanisms, voluntary extensions are also possible, when one economic partner (workers' representative or employer) decides to subscribe to an agreement that it had initially not signed. Therefore, the impact of collective bargaining goes far beyond union membership and the distinction between union and non-union workers or firms becomes largely meaningless.

Collective negotiations are conducted at the industry, or occasionally, at the occupation level. Firm-level negotiation, which for a time was a common practice in large public enterprises, has lost importance. The law does not establish mechanisms of coordination between agreements reached in different negotiations; however, preference is given to verti-

[6]Minimum wage is updated every year by government proposal, taking into account inflation and GDP growth as well as the social partners' expectations.

cal over horizontal agreements, and the principle of the most favorable condition to the worker generally applies.

Since most collective agreements are industry-wide, covering companies with very different sizes and economic conditions, their contents tend to be general, setting minimum working conditions, in particular the base monthly wage for each category of worker, overtime pay, and the normal duration of work.[7] Moreover, only a narrow set of topics is updated annually, and therefore the content of collective agreements is often pointed out as being too immobile and containing little innovation.

Whatever the wage floor agreed upon for each category of worker at the collective bargaining table, firms are free to pay higher wages, and they often deviate from that benchmark, adjusting to firm-specific conditions. Cardoso and Portugal (2005) call this the "wage cushion", the difference between the contractual part of the wage and the actual wage. They estimate that in 1999 actual wages exceeded the level of bargained wages by 20-50%.

4.4 The Data

4.4.1 Quadros de Pessoal data set

It is well established that the nature of the data sets implies the use of different identification strategies and may lead to distinct results. Survey data usually contain more detailed information on observable worker and firm characteristics than administrative data. However, administrative data sets typically cover a long time span, are larger, allow one to follow workers and firms over the years, and just as survey data it enables the use of a control group of non-displaced workers. The use of administrative data in comparison with retrospective survey data reduces recall and reporting errors (Calderwood and Lessof (2009)). Administrative data also

[7]See Hartog et al. (2002) for the effects of bargaining regimes.

usually provide more accurate identification of the timing and nature of the separation arising from firm closure or collective dismissals.

In this study, a longitudinal matched employer-employee data set, called *Quadros de Pessoal* (QP – "Lists of Personnel") is used for the 1997-2008 period. The data are gathered annually by the Portuguese Ministry of Employment, based on an inquiry that every establishment with at least one wage-earner is obliged by law to fill in.[8] Reported data cover the firm, the establishment, and each of its workers.[9] Currently QP gathers information for more than 300,000 firms and about 3 million workers. Given the mandatory nature of the survey plus the fact that these data cover all wage earners in the private sector in Portugal, problems commonly associated with panel data sets, such as panel attrition, are considerably reduced.

Reported data on the worker side include gender, age, schooling, and detailed information on monthly earnings - base wages, regular payments (e.g., seniority), irregular benefits (profits distribution and premiums), overtime payments, and hours of work (normal and overtime). The information on earnings is reported by the employer, which is known to be subject to less measurement error than worker-provided earnings data. All earnings variables were deflated using the Consumer Price Index (with base-year 2008). The notion of job title comes simply from the identification of distinct occupational categories within each collective wage agreement. In Quadros de Pessoal each worker in each year is assigned to the conflation of its professional category and corresponding collective agreement. In each year there are about 300 collective agreements which define wage floors for on average 100 occupational categories. The firm data include detailed information on region, industry, ownership type, and size.

It is worth noting that workers also have an identification number

[8]From 1994 onwards the information refers to the month of October of each year.
[9]See Cardoso(2006) for more details.

based on a (scrambling) transformation of his/her social security number, which allows us to follow them over the years and to match workers and their firms.

4.4.2 Sample Construction

The samples used in this study are selected as in Jacobson et al. (1993) and Couch and Placzek (2010). Thus, we considered displacements due to firm closure, collective dismissals and individual dismissals. In the next section, we explain how firm closures and collective dismissals were identified. To be included in the sample a worker must report positive earnings in the year that immediately precedes the displacement event (reference year is D_0) and must be continuously employed with the same employer during the first three years (screening period).[10] This means that workers are selected into the sample with at least three years of tenure by the time of the reference year. Furthermore, a worker must report positive earnings at least once thereafter, and have known information on their age, gender and education. The sample was restricted to full-time wage earners in the private non-farm sector aged between 20 and 49 years during the final year of the screening period and that were employed in a firm with at least 20 employees (these exclusions reduced the sample size by 21%).[11]

To construct the estimation sample, we proceed as follows. We separate the sample into a control and a treatment group for each possible year of displacement (all years between 2002 and 2006). For example, the

[10]In order to guarantee that the worker was employed with the same employer three years before separation, we control for worker's admission year in the firm. In the year prior to displacement the worker must have at least two years of tenure with the employer.

[11]It should be noted that of the total number of displaced workers due to firm closure, 19%, 18%, 17%, 15% left the firm respectively four, three, two and one year prior to the firm closing down. Thus, it is not possible to observe a clear change in the pattern of early leavers. They might have left for different reasons, e.g. quit or retirement and therefore it was important not to include them in the analysis.

2002 treatment group comprises individuals who were working in 2002 and experienced a displacement event between years 2002 and 2003 (the firm closed down between November 2002 and September 2003).[12] The 2002 control group is the one with those who did not experience any separation between October 2002 and September 2003.

For estimation purposes we define a measure of time relative to the displacement event (D_0). For example, we define D_0 in 2002 for the 2002 displaced group, D_0 in 2003 for the 2003 displaced group, and so on. The data set combines five cohorts (2002-2006) ranging from D_{-6} up to D_6.[13]

Table 4.1: Displacement events in the reference period, 2002-2006

Year	Firm closure	Collective dismissals	Individual dismissals
2002	2591	9755	7552
2003	2121	6593	5448
2004	2008	5368	4638
2005	3100	6250	3806
2006	1579	3576	3084
Total	11,399	31,542	24,528

Notes: This table reports the number of displacement spells per year resulting from firm closure, collective dismissals and individual dismissals, that meet the conditions. The sample includes all displaced individuals who are employed in the year of the displacement D_0 and at least two periods before displacement (D_{-2}) and who are in reemployment in at least one year before the end of the sample period.

As mentioned above, the sample includes all displaced individuals who are employed in the year of the displacement D_0 and at least two periods before displacement (D_{-2}) and who are present in the QP registers in at least one year of the post-displacement period. Table 4.1 reports the number of displacement events in each year. 11,399 displaced due to

[12]Thus, a worker should be identified as displaced in year t if (s)he was employed in year t-1 and experienced a separation between year t-1 and t.

[13]It should be noticed that worker files are not available for the year 2001.

firm closure and 31,542 displaced due to collective dismissals meet these conditions.

Table 4.2: Sample composition, 1997-2008

		Displaced		
Year	Non-displaced	Firm closure	Collective dismissal	Individual dismissal
1997	222576	7379	20503	15508
1998	242560	7764	21812	17069
1999	274808	9249	25566	20056
2000	308367	9547	26000	20485
2002	308006	11312	31455	24524
2003	247774	7621	21864	18027
2004	241190	7374	20039	16722
2005	242018	7576	20373	16675
2006	235030	6903	18734	16420
2007	226502	8012	22489	17613
2008	262536	8810	24432	18794
Total	2,811,367	91,547	253,267	201,893

Notes: This table reports the sample composition in terms of non-displaced and displaced workers resulting from firm closure, collective dismissals and individual dismissals, by year.

After excluding those observations with missing values in the explanatory variables and the extreme values in wages (0.1% top and bottom observations), we obtained a control group composed of 2,811,367 non-displaced worker/year and 91,547 displaced worker/year resulting from firm closure and 253,267 displaced worker/year due to collective dismissals and 201,893 displaced worker/year due to individual dismissals. Table 4.2 reports the number of worker/years in the sample, namely non displaced workers, workers displaced due to firm closure and workers displaced due to collective dismissals. To clarify the link between the two tables, take, for example, firm closures occurring in 2002. 2591 workers were displaced in 2002 due to firm closures (see first line, second column on Table 4.1).

The total number of individuals that were displaced over 2002 and 2006 that were working in 2002 is 11312. The difference between 11312 and 2591 were individuals that experienced a displacement event due to firm closures after 2002 and were, of course, observed in the pre-displacement period.

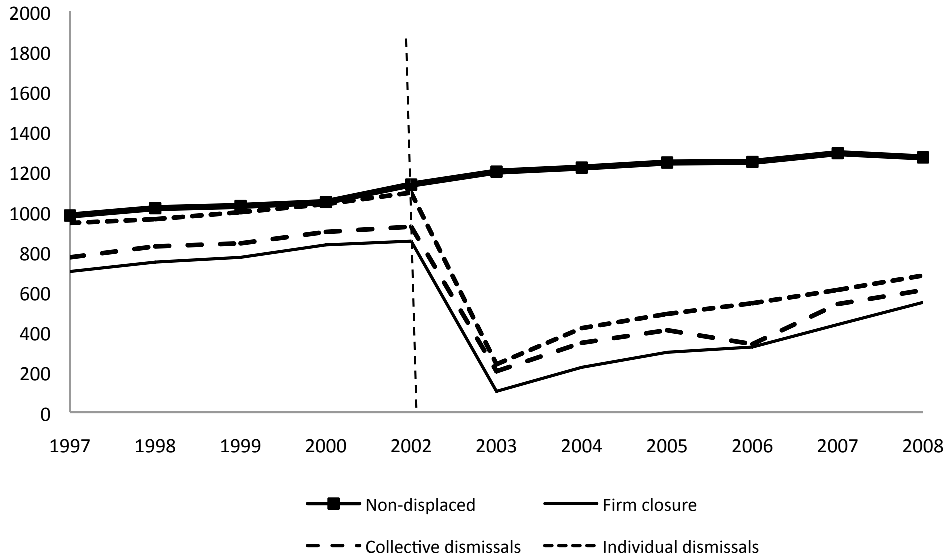
Table 4.6 in Appendix 4B presents the descriptive statistics of the key variables in the data set in the reference year. The statistics are presented separately for the group of displaced and non-displaced workers. Displaced workers are slightly younger, with fewer years of education and tenure in comparison with their non-displaced counterparts. Moreover, the proportion of women is higher in both groups of displaced workers when compared with the group of non-displaced. As expected, firms that close down are smaller and are mainly operating in the sectors of manufacturing and wholesale and retail trade.

A simple descriptive statistics comparison suggests that displaced workers experienced substantial long-term monthly earnings losses. As shown in Figure 4.1, the average monthly earnings of workers that separated in 2002 fell sharply in comparison with their non-displaced counterparts.

4.4.3 Identification of displacements due to firm closure

The data set has a longitudinal dimension, which makes it particularly well suited for analyzing the issues of firms' entry and exit. Each firm entering the database is assigned a unique identifying number and the Ministry implements several checks to ensure that a firm that has already reported to the database is not assigned a different identification number. Using this identifier it is possible to pinpoint all firms that have entered and exited economic activity. In particular, an exit from the database

Figure 4.1: **Monthly earnings of workers separating in year 2002 and non-displaced workers**



Notes: Average monthly earnings (2008 Euros).

should signal a firm that has ceased its activity.[14]

To ensure that we are in the presence of firms' true closures and not mergers or acquisitions, we also excluded from the sample those workers that appeared in the database in the period following displacement with a year of admission in the new job less than the year of displacement minus one.[15] These exclusions reduced the sample size by around 0.1%.

[14]This criteria, however, is not entirely accurate, due to the fact that some of the firms may temporarily exit the database. A temporary exit may occur for a number of reasons other than cessation of activity, a very likely reason being that the survey form was not received in the Ministry of Employment before the date when the recording operations were closed. Almost all of these temporary exits last less than two years, but can still cause an identification problem if they occur in the terminal years. In order to account for this problem, the information on the last two years after displacement was used solely to control for temporary exits in the intermediate years. Thus, a firm is classified as an exiting firm in year t+1 if it is present in year t, but absent in t+1 and t+2. Mergers and acquisitions were used. Only false mergers were excluded.

[15]If, for example, a worker's displacement year is 2002 and (s)he appears in the

Within the reference period, some individuals observe successive spells of firm closure in firms that are necessarily different. For identification purposes, and to be more precise we only used information from the first firm closure within the reference period. Thus, only the first firm closure is used to identify a displacement and the years before and after are used relative to that year of displacement. Thus, the group of displaced workers due to firm closure includes 5 cohorts of workers that lost their jobs between 2002 and 2006.[16]

4.4.4 Identification of displacements due to collective dismissal

To identify a displacement due to a collective dismissal we follow the identification strategy used by Jacobson et al. (1993) and Couch and Placzek (2010). An individual is displaced due to a collective dismissal between t and t+1 if the firm's employment dropped between year t and year t+1, 30 percent or more below its level at year t. The group of displaced workers due to collective dismissal includes 5 cohorts of workers that lost their jobs between 2002 and 2006.

When calculating these employment changes, the magnitude of the flows is much more volatile for small employers. For this reason, and following again Jacobson et al. (1993) and Couch and Placzek (2010), those working for employers with fewer than 20 employees are removed from the sample.

Within the reference period, some individuals observe successive spells of collective dismissals in firms that are necessarily different. For identi-

database in the post-displacement period with a year of admission in the new job of 2001 or earlier, (s)he is excluded from the sample.

[16]It should be noted that of the total number of displaced workers due to firm closure, 19%, 18%, 17%, 15% left the firm respectively four, three, two and one year prior to the firm closing down. Thus, it is not possible to observe a clear change in the pattern of early leavers. They might have left for different reasons, e.g. quit or retirement and therefore it was important not to include them in the analysis.

fication purposes, we only used information from the first collective dismissal within the reference period. Thus, only the first collective dismissal is used to identify a displacement and the years before and after are used relative to that year of displacement.

4.4.5 Identification of individual dismissals

To identify an individual dismissal we follow the identification strategy used by Jacobson et al. (1993) and Couch and Placzek (2010). A worker is displaced due to an individual dismissal between t and t+1 if he separated from a firm where there was no mass layoff or firm closure. The group of displaced workers due to an individual dismissal includes 5 cohorts of workers that separated from their jobs between 2002 and 2006.

When calculating these employment changes, the magnitude of the flows is much more volatile for small employers. For this reason, and following again Jacobson et al. (1993) and Couch and Placzek (2010), those working for employers with fewer than 20 employees are removed from the sample.

Within the reference period, some individuals observe successive spells of individual dismissals in different firms. For identification purposes, we only used information from the first separation within the reference period. Thus, only the first individual dismissal is used to identify a separation and the years before and after are used relative to that year of separation.

4.4.6 Identification of non-displaced workers

The group of non-displaced workers (the control group) includes all individuals that were employed at year t in a firm that did not close in year t+1 and the firm's employment did not drop 30 percent or more and they were not subject to an individual dismissal. The group of non-displaced workers was also restricted to full-time wage earners in the private non-farm sector aged between 20 and 49 years during the final year of the screening period with at least 3 years of tenure and that were employed in a firm with at least 20 employees.

In order to guarantee that the worker was employed with the same employer in the pre-displacement period, we checked the firms identifying number assigned to the worker over that period. These workers were followed over the post-displacement period if they remained with the same employer over that period. Thus, to be included in the sample the worker should appear in at least one of the years between t+1 and t+6.

4.5 Empirical strategy

This section is divided into three sub-sections. The first presents the methodology used by Jacobson et al. (1993). In the second we explore the empirical model with controls for worker, firm and job title observed and unobserved permanent heterogeneity. Later in this section, we show how to disentangle the independent contribution of each fixed effect to the wage losses of displaced workers, using the methodology developed in Gelbach (2010).

4.5.1 Jacobson et al. (1993) statistical specifications

To evaluate the effect of displacement on earnings we use the methodological framework used by Jacobson et al. (1993). The first statistical specification assumes that workers' earnings at a given time period depend on displacement and on some controls for fixed and time-varying characteristics:

$$w_{it} = \alpha_i + \gamma_t + \beta X_{it} + \sum_{k \geq -m} D_{it}^k \delta_k + \epsilon_{it} \qquad (1)$$

where w_{it} represents the earnings (in euros) for each individual i in year t.

Labor earnings are taken as zero whenever the individuals are out of work. D_{it}^k are dummy variables where k is equal to $-m, -(m-1), ..., 0, 1, 2, ...,$ which represent jointly the event of displacement.δ_k represents the effect of displacement on worker's earnings k years prior to, and following, its occurrence, the worker fixed effect, α_i, captures the impact of permanent differences among worker's observed and unobserved characteristics, and γ_t are calendar year fixed effects and they are included to capture the general aggregate time pattern of earnings in the economy. Finally, the vector X_{it} controls for age and age squared. ϵ_{it} is an error term, assumed to be uncorrelated with the covariates. Our identification strategy follows closely the one explored by Jacobson et al. (1993). In a nutshell, we compare the earnings changes of displaced workers over a long-term period with the earnings changes that would have occurred if the displaced had not lost their jobs. Since this latter outcome variable is not observable, a comparison group of non-displaced workers is used. We assume that after controlling for the relevant covariates, the displaced workers would have behaved as the non-displaced in the absence of the displacement event.

The displacement event is assumed to be uncorrelated with the outcome variable. The empirical model that we explore controls for observed and unobserved worker permanent heterogeneity, which is likely to attenuate the source of endogeneity.

Jacobson et al. (1993) used another specification to allow for the possibility that workers have different trend rates of earnings and firms react to these patterns, firing or hiring workers with specific trends. This is modeled by the following equation:

$$w_{it} = \alpha_i + \omega_i t + \gamma_t + \beta X_{it} + \sum_{k \geq -m} D_{it}^k \delta_k + \epsilon_{it} \tag{2}$$

In equation (2) we add to equation (1) a set of "worker-specific time trends", $\omega_i t$.[17]

4.5.2 Individual unobserved fixed effect contribution

It is possible to calculate the independent contribution of each unobserved fixed effect to the monthly wage losses of displaced workers. We use the methodology developed in Gelbach (2010), which appeals to the omitted variables bias formula to compute a detailed decomposition.

To illustrate Gelbach's decomposition, we use the base model with no fixed effects:

$$w_{it} = \gamma_t^{base} + \beta^{base} X_{it} + \sum_{k \geq -m} D_{it}^k \delta_k^{base} + \epsilon_{it}^{base} \tag{3}$$

where δ_k^{base} are the relevant coefficients. This equation has omitted variables bias. It is also necessary to represent the full model with the three fixed effects:

$$w_{ijft} = \hat{\alpha}_i + \hat{\theta}_f + \hat{\lambda}_j + \gamma_t^{full} + \beta^{full} X_{it} + \sum_{k \geq -m} D_{it}^k \delta_k^{full} + \epsilon_{ijft}^{full} \tag{4}$$

This equation adds the three fixed effects to the base model. The base-full difference equals the sample analogue of the omitted variables bias formula. Gelbach's algorithm allows us to decompose the difference $\delta_k^{base} - \delta_k^{full}$ into the separate effect deriving from each excluded variable (each fixed effect). The algorithm is as follows: use ordinary least squares to estimate the vector of coefficients on each covariate in the base model in a set of auxiliary models with each of the three covariates $\hat{\alpha}_i$, $\hat{\theta}_f$, and $\hat{\lambda}_j$ acting as the dependent variable, where all the other variables are used as explanatory variables; this estimate is $\hat{\tau}_k^{\alpha}$, $\hat{\tau}_k^{\theta}$, and $\hat{\tau}_k^{\lambda}$, respectively, for

[17] This specification is estimated by replacing the dependent variable, the time dummies, the Xs, and the displacement dummies by deviations from worker-specific time trends in these variables. In a second step we estimate the resulting model with the detrended variables using ordinary least squares (OLS).

each of the fixed effects.

This algorithm results in decomposing the difference $\delta_k^{base} - \delta_k^{full} = \hat{\tau}_k^{\alpha} + \hat{\tau}_k^{\theta} + \hat{\tau}_k^{\lambda}$, for each time period k.

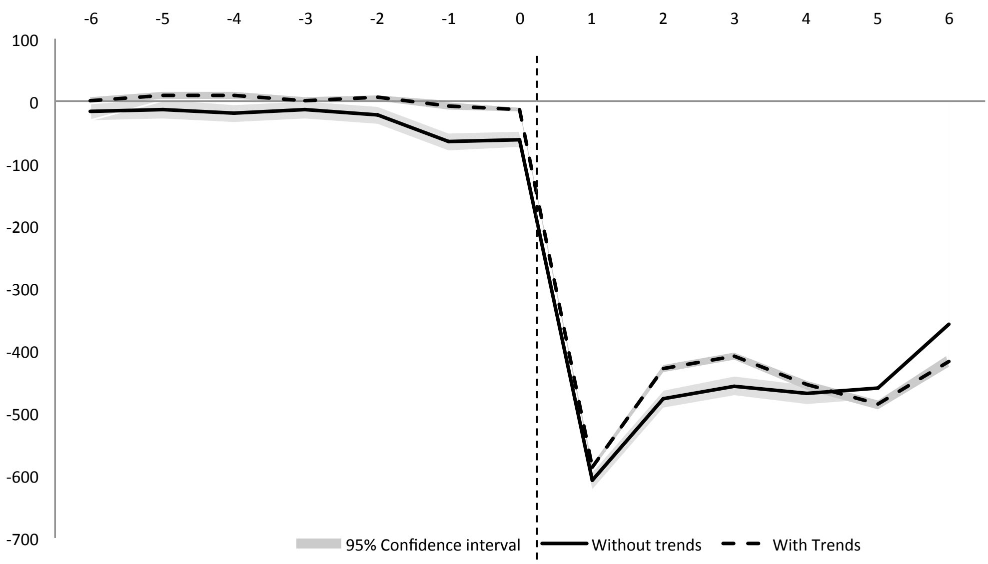
In summary, the decomposition proposed by Gelbach is a computationally simple and econometrically meaningful procedure that takes advantage, in a surprisingly ingenious way, of the conventional OLS omitted variable bias formula. If the base specification is a parsimonious useful benchmark, and in our case it is simply a conditional gross measure of the displacement wage rate losses, the decomposition is also economically meaningful, providing an unambiguous measure of the contribution of each omitted variable (each fixed effect) to the change in the original coefficients of the displacement dummies. For example, the fact that the inclusion of firm fixed effects contributes to decrease the wage loss of displaced workers, simply accounts for the evidence that displaced workers tend to sort themselves into firms that pay, on average, lower wages. When we compare the impact of firm fixed effects before and after displacement, we are simply isolating the dominant influence of movements from higher paying firms into lower paying firms. A similar interpretation applies to the role of job-title fixed effects.

4.6 Empirical Results

The results from the estimation of the JLS model, described in equations (1) and (2), are summarized in Figure 4.2. In accordance with the individual trend specification, the monthly earnings losses amount to 587 Euros (72 percent of average pre-displacement wages) 1 year after the shutdown of the firm, and are attenuated to 416 (51 percent), 6 years after displacement.[18]

[18] As for the fixed-effects specification, the monthly earnings losses amounted to 608 euros (74 percent of average pre-displacement wages) 1 year after the shutdown of the firm, and decreased to 356 (44 percent), 6 years after displacement.

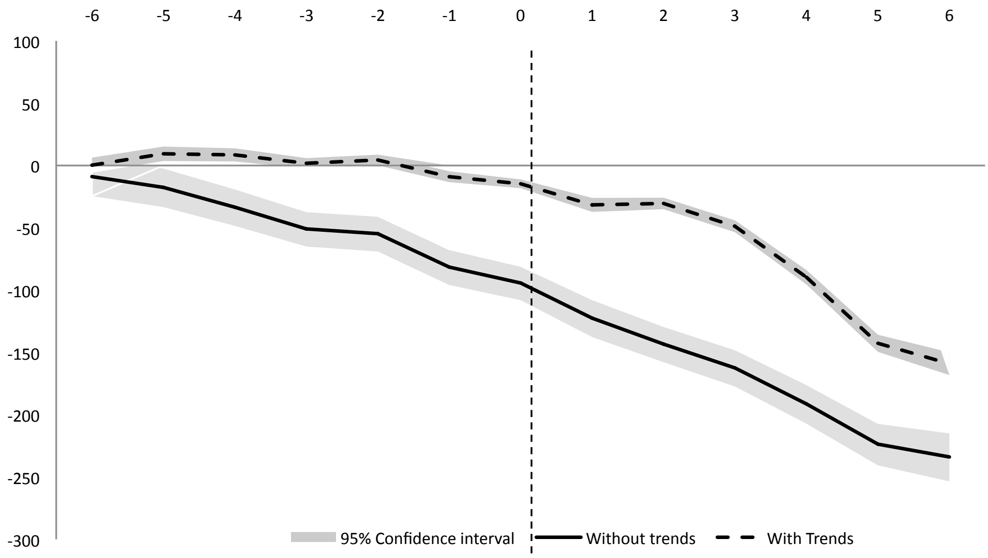
Figure 4.2: **Monthly earnings loss of displaced workers due to firm closure**



Notes: Monthly earnings losses, including transitions to zeros (2008 Euros). In the horizontal axis, the relative time to firm closure is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects.

The size of the loss is largely driven by the joblessness experience of the displaced workers, where, in accordance with JLS, labor earnings are taken as zero whenever the individuals are out of work. The upswing of earnings after the first year of displacement is generated mostly by the reemployment of workers. Conditional on being displaced and returning, 18 percent of the individuals return in the first year, 27 percent return after 2 years, 23 percent return after 3 years, 14 percent return after 4 years, 11 percent return after 5 years, and 7 percent return after 6 years. Indeed, the impact of reemployment in earnings recovery more than offsets the significant monthly wage rate of displaced workers documented below. Note that the estimates produced by the fixed effects and the random trend models are identical, as in Couch and Placzek (2010). In contrast with Jacobson et al. (1993) but in line with Couch and Placzek (2010), we fail to observe a severe earnings dip prior to displacement. In our individual-trend specification there is no indication that earnings had fallen before the firm closure. For the fixed-effects specification, however, there is some evidence that earnings fell modestly.

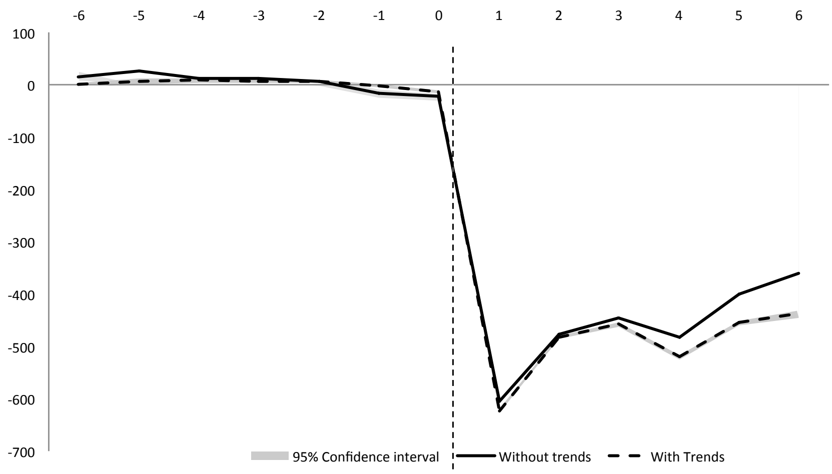
Figure 4.3: **Monthly wage loss of displaced workers due to firm closure**



Notes: Monthly wage losses, excluding transitions to zeros. In the horizontal axis, the relative time to firm closure is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects.

The results from the estimation of the deepen analysis from equations (1) and (2) therefore excluding the joblessness events are summarized in Figure 4.3. When we restrict our analysis to the profile of monthly wages before and after displacement, we find that wage rates started declining one year before the shutdown of the firm and continued to decline for up to five years after firm closure, reaching 17 percent (27 percent) in the case of the random trend (fixed effects) specification. At least three mechanisms may be a work. First, it may be that workers who found relatively higher wage offers returned earlier to employment. Second, longer joblessness duration may have impaired the human capital of displaced workers. And third, it may take some time for unemployed individuals to realize that their expectation about the relevant wage offer distribution is unrealistic, in particular in a labor market where the potential duration of unemployment benefits is very generous (reaching up to 57 months).

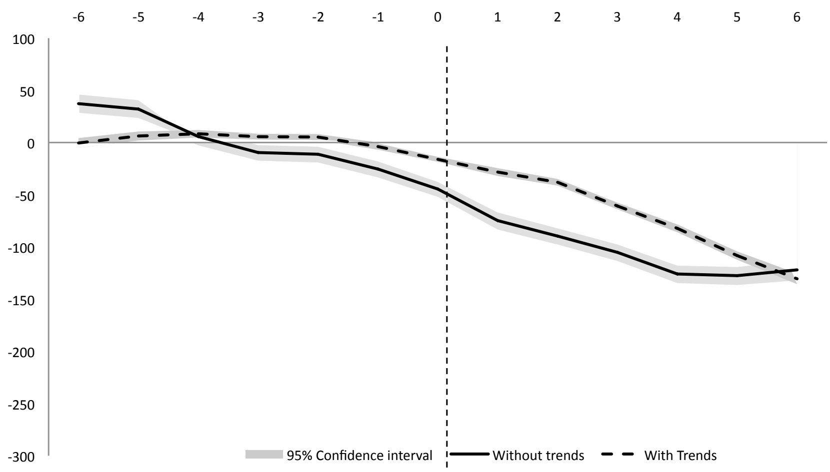
Figure 4.4: **Monthly earnings loss of displaced workers due to collective dismissals**



Notes: Monthly earnings losses, including transitions to zeros (2008 Euros). In the horizontal axis, the relative time to separation through a collective dismissal is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects.

When we repeat the same analysis for collective dismissals, we find broadly similar results (see Figure 4.4). One year after the separation, earnings fell by 631 Euros (605 Euros), which corresponds to 69 percent (67 percent) of average earnings, for the random trend (fixed effects) specification. Conditional on returning, 16 percent of the individuals return in the first year, 23 percent return after 2 years, 21 percent return after 3 years, 16 percent return after 4 years, 14 percent return after 5 years, and 10 percent return after 6 years. Here, not even a small fall on earnings prior to separation is observed. The overall shape of the evolution of wage rates again mimics those observed for firm closures, even if the fall is not as large (see Figure 4.5). Wage rates decline 14 percent for the random trend model and 14 percent for the fixed-effects model, six years after displacement.

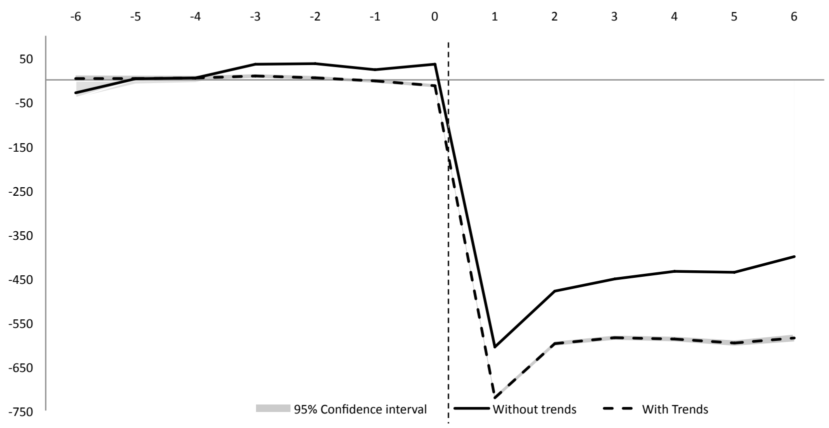
Figure 4.5: **Monthly wage loss of displaced workers due to collective dismissals**



Notes: Monthly wage losses, excluding transitions to zeros. In the horizontal axis, the relative time to separation through a collective dismissal is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects.

Figures 4.6 and 4.7 replicate the previous exercise using a third sample group based on workers that separated due to an individual dismissal. Workers displaced due to a collective dismissal contrast with the ones from individual dismissals because their separation is by definition involuntary. One year after the separation, monthly earnings fell by 721 Euros (606 Euros), which corresponds to 54 percent (64 percent) of average earnings, for the random trend (fixed effects) specification. The fall in percentage was smaller for this group. In Figure 4.7 one year after displacement workers that lost their job due to an individual dismissal observed virtually no change in their monthly wages. Only after the third year their monthly wages start falling. Thus, the loss in earnings was made almost entirely through the joblessness spell.

Figure 4.6: **Monthly earnings loss of displaced workers due to individual dismissals**

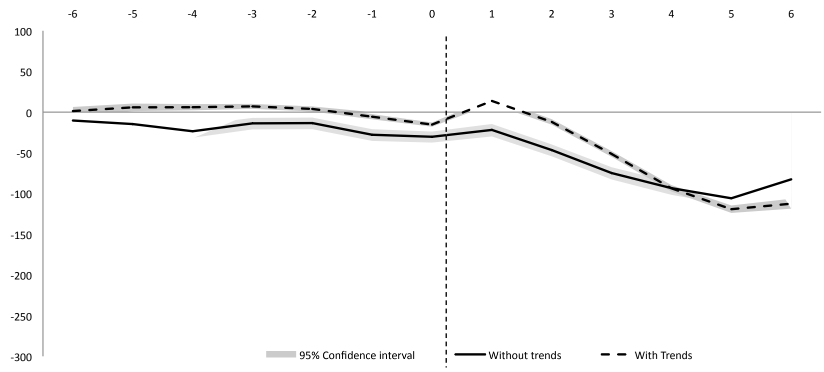


Notes: Monthly earnings losses, including transitions to zeros (2008 Euros). In the horizontal axis, the relative time to separation through an individual dismissal is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects.

To better understand the nature of the wage rate changes that affected displaced workers in comparison to non-displaced workers, we turn to the estimation of the three-way high-dimensional fixed effects regression model as given in equation (5). Computation of the three fixed effects is based on all the wage earners observed between 1986 and 2008, corresponding to 28,212,770 observations. The interpretation of the parameters of this model is straightforward and the decomposition exercise enabled by it, that is, the role of worker, firm, and job title heterogeneity - is discussed at length by Torres et al. (2012).

After restricting the data set to the group of displaced workers due

Figure 4.7: **Monthly wage loss of displaced workers due to individual dismissals**

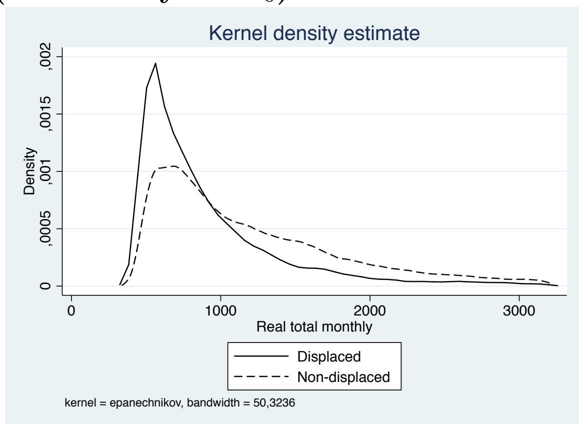


Notes: Monthly wage losses, excluding transitions to zeros. In the horizontal axis, the relative time to separation through an individual dismissal is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects.

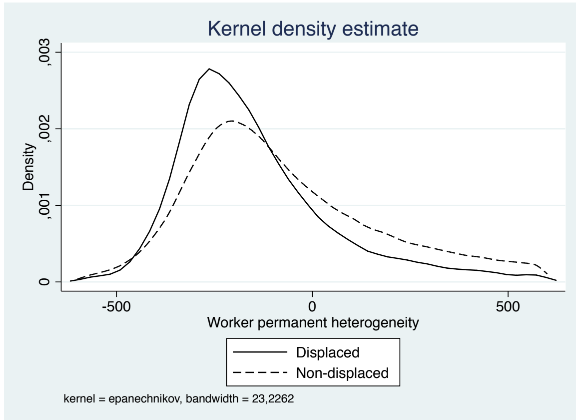
to firm closure and their control group of non-displaced workers, we end up with a longitudinal sample of 2,811,367 worker-year observations. We start by graphing the empirical (log)wage distributions of workers displaced due to firm closures and their non-displaced counterparts in Figure 4.8 (a). It is clear that the wages of displaced workers are lower (28 percent, on average) and less dispersed when compared with those of the non-displaced. The overall shape of the wage distribution can be better understood by looking at the distributions of the worker, firm, and job title fixed effects.

Figure 4.8 (b) depicts the empirical distribution of permanent worker heterogeneity, both observed (such as gender or schooling) and unobserved. A high worker fixed effect (high-wage worker) is an individual with total compensation higher than expected on the basis of observable time-varying regressors and for the heterogeneity of firms and job titles. A distinction is made between continuing and destroyed matches due to firm closures. The graph is based on the estimation of 409,687 worker fixed

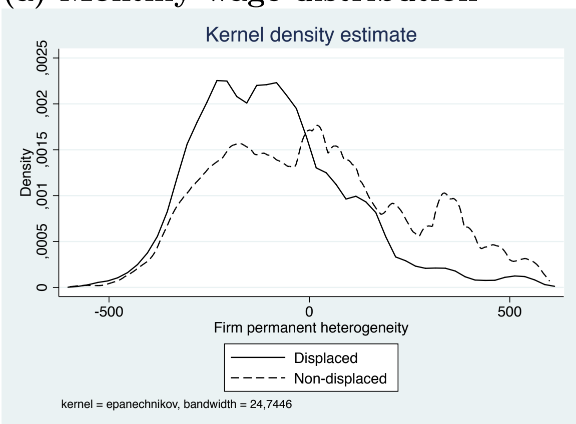
Figure 4.8: **The empirical distribution of wages pre-displacement (reference year D_0)**



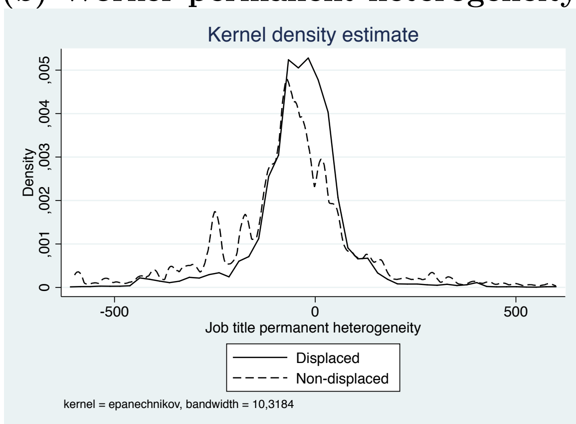
(a) Monthly wage distribution



(b) Worker permanent heterogeneity



(c) Firm permanent heterogeneity



(d) Job title permanent heterogeneity

Notes: This figure plots the empirical distributions of different variables before displacement of workers displaced due to firm closures and their non-displaced counterparts (reference year D_0).

effects. Not surprisingly, the shape of the distributions closely resembles the distributional shape of log wages. The linear correlation between log wages and worker fixed effects is 0.75. From comparison between displaced and non-displaced workers it is clear that those workers who exited their firms have permanent (observed and unobserved) characteristics that are associated with significantly lower wages.

Less well studied is the heterogeneity of wage policies across firms. In Figure 4.8 (c) we present the empirical distribution of the 33,390 firm fixed effects. A high firm fixed effect (high-wage policy from the firm) is a firm with total compensation higher than expected on the basis of observable time-varying regressors and for the heterogeneity of workers and job titles. The role of firm heterogeneity on wage formation is quite important. The linear correlation coefficient between log wages and firm fixed effects is no less than 0.69. Not surprisingly, the comparison between the two distributions shows that displaced workers earned much lower wages in large part because the firms from which they separated exhibited a less generous wage policy. On average the firm fixed effect attached to those displaced workers is 63% less than those of the control group.

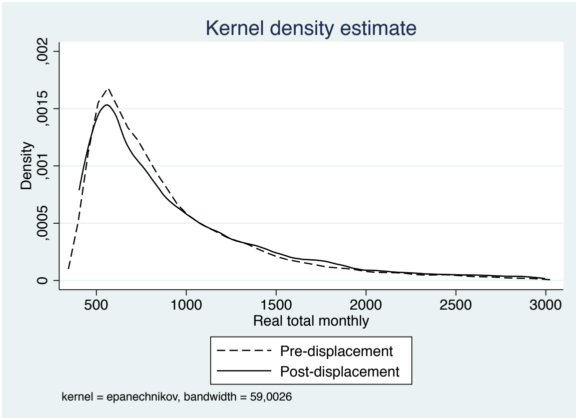
The heterogeneity of job title fixed effects is likely to be generated by variations across occupations and skills and by differences across collective wage agreements. As discussed above, the notion of job title comes simply from the identification of distinct occupational categories within each collective wage agreement. Over the years of the survey we could estimate 46,295 job title fixed effects. A high job title fixed effect (job title premium) is a job title with total compensation higher than expected on the basis of observable time-varying regressors and for the heterogeneity of workers and firms. Job title heterogeneity has a non-trivial impact on the determination of wages. The linear correlation between job title fixed effects and wages is a respectable 0.45. From panel (d) in Figure 4.8 it is clear that prior to firm closure displaced workers filled positions that were paid above those of the non-displaced.

In Figure 4.9 we compare the distribution of wages (and its components) of displaced workers before and after displacement. Panel (a) of the figure shows that the distribution of wages was shifted to the left, evincing significant wage losses associated with firm closures. Panel (b) exhibits the worker fixed effect distribution. Except for the self-selection generated by different timing of reemployment, the two distributions should coincide exactly, which for the most part they do, suggesting that the time profile of reemployment is not a serious concern, at least in the worker heterogeneity dimension. Panels (c) and (d) both reveal that workers moved to lower paying firms and job titles, especially, in the right tail of the two distributions. In Appendix, Figure 4.12 is based in Figure 4.9 which allows me to characterize better, who moves down and who moves up in all sources. 56% of displaced workers move to worse paying firms while 44% moves to job titles that are worse paid, 25% do not change job titles and 31% move to job titles that are better paid than their job title pre-displacement.

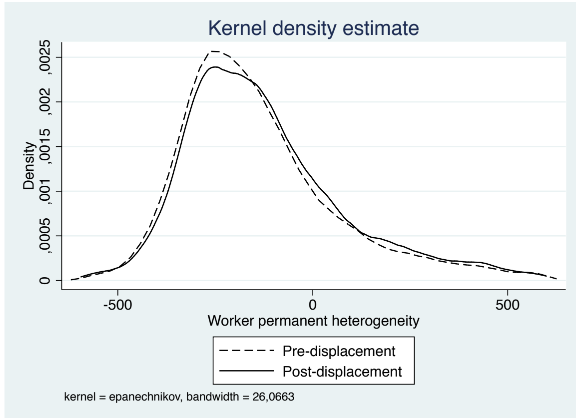
To investigate how the coefficient estimates of the displacement dummies change with the inclusion of three fixed effects we implemented the conditional decomposition method suggested by Gelbach (2010), discussed above. As hinted earlier, this procedure allows us to unambiguously disentangle the contribution of each excluded variable (each fixed effect) to the change in the coefficient estimate of the variables under scrutiny.

The results of the Gelbach decomposition are seen in Table 4.3. The first two columns of the table give the coefficient estimates for the benchmark OLS regression and for a regression that includes, in addition, the three fixed effects. Thus, in the fourth line of the tables we can see that three years prior to the shut-down of the firm, displaced workers received wages that were 298.7 euros below those of the non-displaced. Once we account for worker, firm, and job title fixed effects, the remaining unexplained difference in wages falls to -18.4 euros. This means that the inclusion of the fixed effects accounts for 280.3 euros of the difference between the wages of displaced and non-displaced workers, where 141.9 euros are accounted for by the worker fixed effect, 184 euros are accounted for by the firm fixed effect, and 46.2 euros are accounted for by the job title fixed effect. On average, after displacement, the difference in wages is 61.1 percent (next to the last line). Worker heterogeneity is responsible

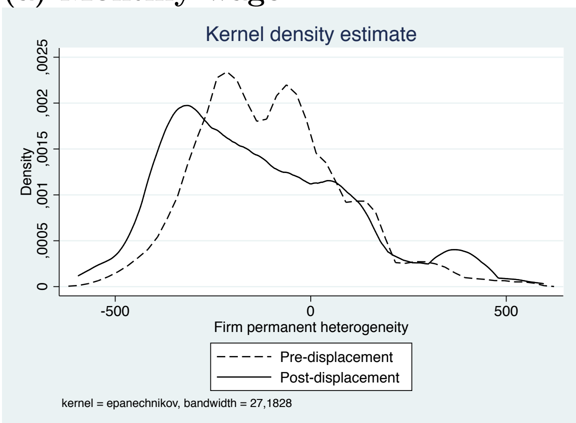
Figure 4.9: **The empirical distribution of wages of displaced workers: pre- and post-displacement**



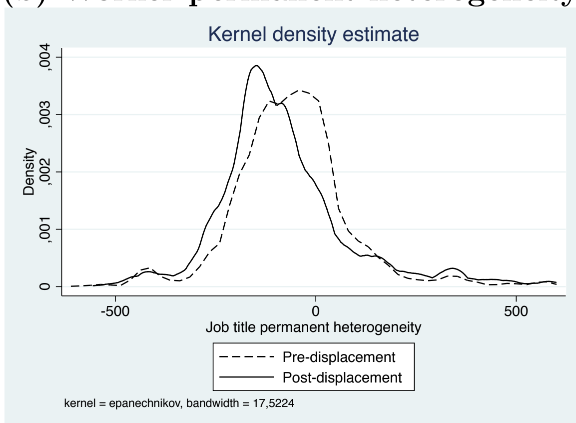
(a) Monthly wage



(b) Worker permanent heterogeneity



(c) Firm permanent heterogeneity



(d) Job title permanent heterogeneity

Notes: Displaced workers' density distributions one year before displacement and one year after displacement.

for 20.4 percentage points, firm heterogeneity explains 33.6 percentage points, and 6.4 percentage points are related with job title heterogeneity, totaling 60.3 percentage points. When we look at average differences in the periods before and after displacement, we arrive at a wage loss of 22.1 percent (last line). The three fixed effects account for 20.6 percentage, which can be disentangled as 2 percentage points due to worker composition, 8.2 percentage points due to sorting into lower paying firms, and

10.5 percentage points due to sorting into lower paying job titles.[19]

It makes sense to admit that wages also depend on job tenure. We performed the decomposition including tenure. The results are in Appendix on Table 4.16. Tenure is not significant and the other results do not change significantly.[20]

Table 4.3: **Decomposition of the wage loss - displaced workers due to firm closure**

Period relative to displacement	Base OLS monthly wage	Full OLS monthly wage	$\delta_k^{base} - \delta_k^{full}$	Worker fixed effect	Firm fixed effect	Job title fixed effect
D_{-6}	-270.2	17.6	-287.9	-135.7	-212.9	62.3
D_{-5}	-278.2	8.8	-287.0	-130.4	-213.4	58.0
D_{-4}	-295.4	-2.5	-292.8	-152.8	-181.0	41.7
D_{-3}	-298.7	-18.4	-280.3	-141.9	-184.0	46.2
D_{-2}	-322.0	11.1	-333.1	-151.0	-214.8	33.2
D_{-1}	-395.5	-10.5	-384.9	-168.1	-220.5	4.0
D_0	-376.1	32.1	-408.2	-172.8	-229.1	-6.1
D_1	-421.2	-12.7	-408.5	-137.4	-237.8	-33.2
D_2	-492.6	-6.1	-486.5	-178.8	-253.0	-54.6
D_3	-514.6	3.9	-518.4	-185.8	-264.8	-67.9
D_4	-574.7	-10.1	-564.6	-198.8	-300.5	-65.4
D_5	-508.0	19.3	-527.3	-180.5	-290.6	-56.5
D_6	-492.3	-35.4	-456.9	-119.3	-302.3	-35.4
$D_{-6} - D_0$	-319.4	5.4	-324.9	-150.4	-207.9	34.2
$D_1 - D_6$	-500.6	-6.8	-493.7	-166.8	-274.8	-52.2
Δ	-181.1	-12.3	-168.8	-16.4	-66.9	-86.3
Results in percentage						
$D_{-6} - D_0$	-39.0	0.7	-39.7	-18.4	-25.4	4.2
$D_1 - D_6$	-61.1	-0.8	-60.3	-20.4	-33.6	-6.4
Δ	-22.1	-1.5	-20.6	-2.0	-8.2	-10.5

Notes: This table reports the Gelbach decomposition of the three fixed effects of the wage loss of displaced workers. In the regressions we control for age and age squared and calendar year fixed effects. In each column, $D_{-6} - D_0$ is the computed average between the first seven lines (D_{-6} to D_0). $D_1 - D_6$ is the computed average between the next six lines (D_1 to D_6). In the line Δ we compute the difference between the previous two lines. In the last three lines we compute the results in percentage by dividing the respective numbers by the average wage of displaced workers in the pre displacement period (819 euros).

[19] The contribution of the worker fixed effect can be taken as an indication that self-selection (at least the one that is based on the observable and unobservable permanent characteristics of the worker) does not play a dominant role.

[20] Results with the tenure effect when the fixed effects are removed were included in Appendix. Tenure accounts for a wage loss of 5.1% (Table 4.16). Once fixed effects are included in the wage regression the loss associated with job tenure is reduced to 0.3% (Table 4.15).

This suggests that the most important factor driving the wage penalty of these displaced workers is the fact that they are reemployed into job categories (and or collective agreements) that are less generously remunerated. The unfavorable allocation into job titles accounts for roughly half of the total average wage loss. Sorting into firms also plays an important role, accounting for one third of the total average wage loss.[21]

Workers in our sample affected by collective dismissals faced a much lower wage penalty (12 pp) than those that suffered from a firm closure (see Table 4.14). As before, sorting into job titles is the most influential factor, accounting for 54 percent of the total average loss. The allocation into firms with different wage policies, however, explains 44 percent of the loss. Only a small part of the wage penalty (0.9 pp) could not be accounted for.

Workers in our sample affected by individual dismissals faced an even lower wage penalty (8.5 pp) than those that suffered from a firm closure (see Table 4.5). Contrary to the previous results, sorting into firms is the most influential factor, accounting for 71 percent of the total average loss. The unfavorable allocation into job titles does not play a significant role in explaining the loss of these workers. Only a small part of the wage penalty (0.3 pp) could not be accounted for.

[21]The wage change (in particular that associated with the job title and the firm fixed effect) can be generated by rents or compensating wage differentials, as discussed above. In the first case a wage drop after displacement can be interpreted as a welfare loss to the worker, in the second case there may be no welfare loss.

Table 4.4: **Decomposition of the wage loss - displaced workers due to collective dismissals**

Period relative to displacement	Base OLS monthly wage	Full OLS monthly wage	$\delta_k^{base} - \delta_k^{full}$	Worker fixed effect	Firm fixed effect	Job title fixed effect
D_{-6}	-194.9	6.6	-201.5	-86.1	-179.8	65.1
D_{-5}	-229.7	-1.5	-228.2	-99.3	-191.0	63.0
D_{-4}	-220.3	-4.6	-215.7	-96.0	-176.5	57.4
D_{-3}	-212.1	-9.1	-203.1	-87.7	-168.4	53.5
D_{-2}	-232.1	0.3	-232.4	-95.7	-178.0	41.8
D_{-1}	-285.6	-10.5	-275.2	-107.2	-189.2	21.6
D_0	-277.0	26.5	-303.5	-117.4	-196.0	10.2
D_1	-226.2	-12.4	-213.8	-39.8	-171.4	-2.5
D_2	-307.8	-14.4	-293.4	-80.5	-201.3	-11.6
D_3	-366.3	-0.4	-365.9	-107.0	-230.4	-28.5
D_4	-459.7	-0.3	-459.4	-145.1	-277.0	-37.4
D_5	-365.3	-12.4	-352.8	-121.0	-244.2	12.2
D_6	-342.2	-4.8	-337.4	-111.8	-235.1	9.3
$D_{-6} - D_0$	-236.0	1.1	-237.1	-98.5	-182.7	44.7
$D_1 - D_6$	-344.6	-7.4	-337.1	-100.9	-226.5	-9.7
Δ	-108.6	-8.6	-100.1	-2.4	-43.8	-54.4
Results in percentage						
$D_{-6} - D_0$	-26.1	0.1	-26.2	-10.9	-20.2	4.9
$D_1 - D_6$	-38.1	-0.8	-37.3	-11.1	-25.0	-1.1
Δ	-12.0	-0.9	-11.1	-0.3	-4.8	-6.0

Notes: This table reports the Gelbach decomposition of the three fixed effects of the wage loss of displaced workers. In the regressions we control for age and age squared and calendar year fixed effects. In each column, $D_{-6} - D_0$ is the computed average between the first seven lines (D_{-6} to D_0). $D_1 - D_6$ is the computed average between the next six lines (D_1 to D_6). In the line Δ we compute the difference between the previous two lines. In the last three lines we compute the results in percentage by dividing the respective numbers by the average wage of displaced workers in the pre displacement period (905 euros).

Table 4.5: **Decomposition of the wage loss - displaced workers due to individual dismissals**

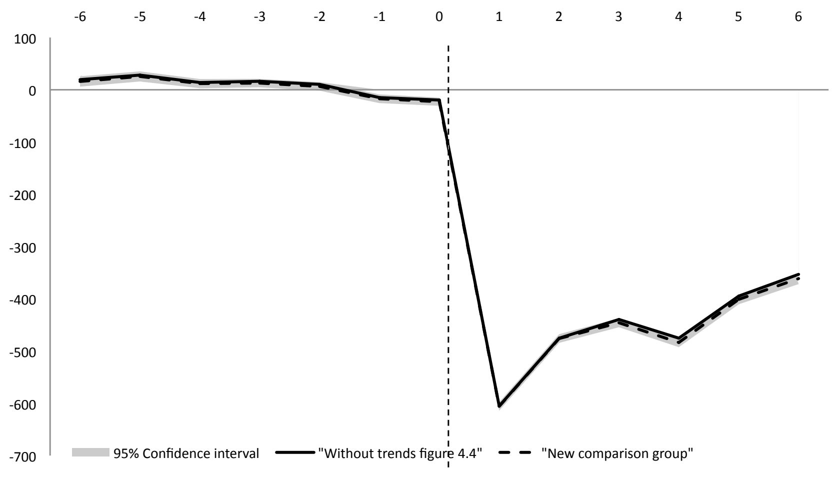
Period relative to displacement	Base OLS monthly wage	Full OLS monthly wage	$\delta_k^{base} - \delta_k^{full}$	Worker fixed effect	Firm fixed effect	Job title fixed effect
D_{-6}	-45.0	-24.2	-20.8	32.4	-86.8	34.2
D_{-5}	-33.2	-18.7	-14.4	39.5	-88.6	34.8
D_{-4}	-4.9	-13.5	8.6	46.1	-65.6	27.9
D_{-3}	-32.7	-21.5	-11.2	29.1	-69.5	29.3
D_{-2}	-29.1	0.8	-29.9	24.6	-81.9	27.8
D_{-1}	-76.4	-22.0	-54.3	16.4	-89.9	19.5
D_0	-17.2	75.0	-92.2	-5.1	-101.7	14.8
D_1	44.1	4.1	40.1	92.0	-91.8	39.9
D_2	-71.9	-0.7	-71.2	30.4	-127.1	25.4
D_3	-142.8	-10.6	-132.2	9.0	-153.5	12.2
D_4	-185.5	-9.9	-175.6	-12.1	-177.1	13.2
D_5	-218.2	-12.6	-205.6	-31.2	-182.1	7.2
D_6	-204.7	31.4	-236.1	-42.1	-191.4	-2.9
$D_{-6} - D_0$	-34.1	-3.5	-30.6	26.1	-83.4	26.9
$D_1 - D_6$	-129.8	0.3	-130.1	7.7	-153.8	15.9
Δ	-95.7	3.7	-99.5	-18.5	-70.4	-11.0
Results in percentage						
$D_{-6} - D_0$	-3.0	-0.3	-2.7	2.3	-7.4	2.4
$D_1 - D_6$	-11.5	0.0	-11.6	0.7	-13.7	1.4
Δ	-8.5	0.3	-8.8	-1.6	-6.3	-1.0

Notes: This table reports the Gelbach decomposition of the three fixed effects of the wage loss of displaced workers. In the regressions we control for age and age squared and calendar year fixed effects. In each column, $D_{-6} - D_0$ is the computed average between the first seven lines (D_{-6} to D_0). $D_1 - D_6$ is the computed average between the next six lines (D_1 to D_6). In the line Δ we compute the difference between the previous two lines. In the last three lines we compute the results in percentage by dividing the respective numbers by the average wage of displaced workers in the pre displacement period (1126 euros).

4.6.1 Sensitivity of losses to comparison group

The idea to use a comparison group in the framework proposed by Jacobson et al. (1993) is to estimate the earnings changes that would have occurred if there was no displacement. Instead of using all non-displaced workers we can use the co-workers that were in the same firm where the displacement occurred. In this section we compare displaced workers' earnings to those of non-displaced workers in the same firm. These workers are a better comparison group because they are more similar to the displaced workers. In Figures 4.10 and 4.11 we replicate respectively Figures 4.4 and 4.6 using this new comparison group. We see that results are not affected by the use of this new comparison group.

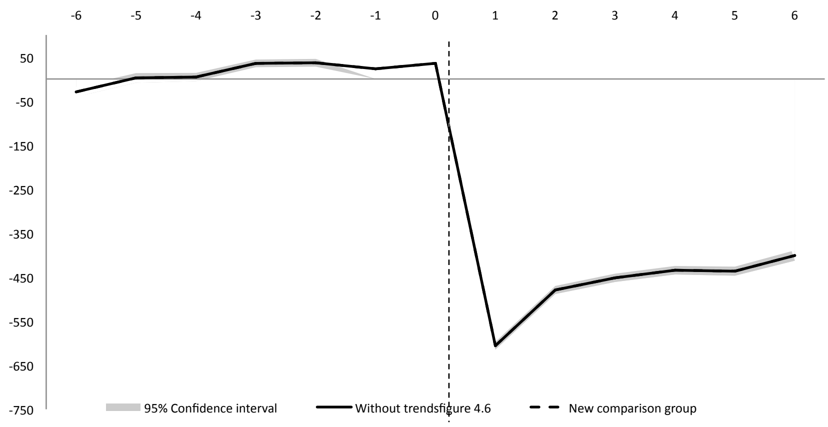
Figure 4.10: **Sensitivity of monthly earnings losses of collective dismissals to different comparison group**



Notes: Monthly earnings losses, including transitions to zeros (2008 Euros). In the horizontal axis, the relative time to separation through a collective dismissal is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects. Both lines are based in the model from equation (1).

These findings suggest that stayers in firms that had individual or even

Figure 4.11: **Sensitivity of monthly earnings losses of individual dismissals to different comparison group**



Notes: Monthly earnings losses, excluding transitions to zeros. In the horizontal axis, the relative time to separation through an individual dismissal is plotted in years. In the regressions we control for age and age squared and calendar year fixed effects. Both lines are based in the model from equation (1).

collective dismissals do not observe any changes in their wages (Jacobson et al. (1993)). The separations of other workers do not create any pressure for the earnings of workers that remain working at the firm. These results reinforce the quality of the comparison group used in the rest of the chapter.

4.7 Concluding remarks

Using a rich matched employer-employee data set for Portugal, we studied the persistent earnings losses of workers displaced due to firm closure, collective dismissals and individual dismissals. We found that those losses are rather severe and persistent, representing 51 percent (48 percent and 52 percent) of the pre-displacement wages for firm closures (collective dismissals and individual dismissals), six years after the separation event. Those losses are largely explained by the joblessness experience of the displaced workers, during which labor earnings are absent. Wage rates also tumble for displaced workers, in comparison with non-displaced workers, amounting to a 19 percent monthly wage fall in the case of firm shut downs, and 14 percent in the case of collective dismissals, and 10 percent in the case of individual dismissals, six years after displacement.

Potential losses of displaced worker can be related to the firm and job-title that they held before and after displacement. We thus explored the sources of those losses, estimating a three-way high-dimensional fixed effects regression model, which enabled us to obtain worker, firm, and job title fixed effects. To investigate the estimates of the wage losses with the inclusion of three fixed effects, we implemented the conditional decomposition method suggested by Gelbach (2010). We found that the allocation into lower-paid job titles plays the most important role in explaining the wage losses of displaced workers, accounting for half of the total average wage loss in the case of firm closure, and 54 percent in the case of collective dismissals, but not in the case of individual dismissals, where it accounts only for 11 percent of the loss. Given the way those job titles were identified, they may also reflect the bargaining power of the workers. Because job-titles contain the skill requirements of the position held by the worker, it will also retain the hierarchical standing of the workers. Again, with the event of a displacement, a human capital will be destroyed, largely associated with the loss of his pre-displacement job-title.

Sorting into firms also plays a significant role for workers displaced through firm closures, accounting for 40 percent of the total average wage loss, and 44 percent in the case of collective dismissals, and 71 percent of the loss in the case of individual dismissals. Different wage policies are favored by the existence of industry rents (due to unionization or incentive pay premiums) or the operation of wage efficiency policies. In such an environment, the worker may benefit from engaging in job search to locate the firms with more suitable (more generous) wage offers. However, with the occurrence of a displacement event, successful job searchers may loose their "job shopping" investment.

There are important policy prescription that may be derived from the evidence given in the chapter. Severe losses in the returns to the job-title may represent a job downgrading due to depreciation of general specific human capital. Here, retraining programs may be of some help. Losses related with the firm fixed effect may mean that a worker is moving from a "good" match to a "bad" match. If phenomenon is pervasive, job search assistance programs and mandatory pre- notification of impending redundancy may be justified.

4.8 Appendix

Appendix A - Description of variables

Firm closure: A firm closure is observed if the identification number of one firm appeared in period t but did not appear in t+1 and t+2.

Collective dismissals: Firms where employment has declined by at least 30 percent. This reduces the likelihood that voluntary leavers and workers fired for cause are included in the sample.

Individual dismissals: A worker is displaced due to an individual dismissal between t and t+1 if he separated from a firm where there was no mass layoff or firm closure.

Total monthly earnings: Labor earnings that are a combination of several components: base wage, regular payments (e.g., seniority and transportation), irregular benefits (profits and premium), and overtime hours payments.

Hourly wage: Ratio between total monthly earnings and total hours of work (normal+overtime) in real euros, measured in logarithms.

Tenure: Number of years an employee has worked for his firm.

Age: Age of the individual measured in years.

Education level: Six education categories were defined: (1) Less than Basic School, which includes individuals with fewer than 4 years of schooling, (2) Basic School, which includes individuals with 4 completed years of schooling, (3) Preparatory, which includes individuals with 6 completed years of schooling, (4) Lower Secondary, which includes individuals with 9 completed years of schooling, (5) Upper Secondary, which includes individuals with secondary schooling and (6) College, which includes individuals with at least a bachelor degree.

Firm size: The number of workers currently working in the firm, measured in logarithm.

Industry: Six categories were defined: (1)Manufacturing, (2)Construction, (3)Wholesale and retail trade, (4)Transports, (5)Finance and business services, and (6)Education and Health.

Appendix B

Table 4.6: Descriptive statistics in reference year (2002)

	Non-displaced	Firm closure	Collective dismissals	Individual dismissals
Age (years)	37	35	34	34
Tenure (years)	11	10	9	8
Female	40	46	46	35
Total monthly wage (2008 euros)	1136	819	905	1126
Minimum monthly wage (2008 euros)	408	408	408	408
Hourly wage (2008 euros)	2,08	1,49	1,64	2,07
Education (percentages):				
Less than basic school	1	1	1	1
Basic school	23	30	27	17
Preparatory	23	33	30	23
Lower Secondary	19	15	16	19
Upper Secondary	23	15	19	27
College	11	6	7	14
Firm size (no. co-workers)	1460	195	567	1391
Industry (percentages):				
Manufacturing	41	60	53	37
Construction	7	13	12	9
Wholesale and retail trade	20	17	18	30
Transports	10	2	7	5
Finance and business services	13	7	8	16
Education and Health	9	1	2	3
No. Observations	308,006	11,312	31,455	24,524

Notes: This table reports summary statistics (mean) for the reference year used in the analysis to construct the sample. The second column shows statistics computed using non displaced workers (control group) and on the third, fourth and fifth columns they are computed using the sample of displaced workers resulting from firm closure and collective dismissals and non-mass layoff dismissals (treatment groups). Variables represented are those described in detail in Appendix A. The units are explained in front of the variables while gender, education and industry are shown as a percentage.

Appendix C

Table 4.7: **Detailed results from Figure 4.2**

without trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]	with trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	-15.1	8.1	-1.9	0.1	-31.0	0.8	D_{-6}	0.3	3.3	0.1	0.9	-6.1	6.7
D_{-5}	-12.3	8.2	-1.5	0.1	-28.3	3.7	D_{-5}	9.3	3.1	3.0	0.0	3.2	15.4
D_{-4}	-19.1	7.7	-2.5	0.0	-34.2	-3.9	D_{-4}	8.4	2.8	3.0	0.0	2.8	13.9
D_{-3}	-13.1	7.3	-1.8	0.1	-27.5	1.2	D_{-3}	2.0	2.2	0.9	0.4	-2.3	6.4
D_{-2}	-22.9	7.4	-3.1	0.0	-37.3	-8.4	D_{-2}	4.9	2.3	2.2	0.0	0.5	9.4
D_{-1}	-64.7	7.4	-8.7	0.0	-79.3	-50.2	D_{-1}	-7.8	2.3	-3.3	0.0	-12.4	-3.2
D_0	-60.5	7.1	-8.5	0.0	-74.5	-46.6	D_0	-13.6	1.9	-7.2	0.0	-17.4	-9.9
D_1	-607.7	7.4	-82.7	0.0	-622.1	-593.2	D_1	-586.6	2.3	-255.2	0.0	-591.1	-582.0
D_2	-476.2	7.4	-64.1	0.0	-490.8	-461.6	D_2	-428.5	2.3	-183.6	0.0	-433.1	-423.9
D_3	-455.7	7.7	-59.3	0.0	-470.8	-440.7	D_3	-408.0	2.5	-160.7	0.0	-412.9	-403.0
D_4	-468.8	8.1	-57.7	0.0	-484.7	-452.8	D_4	-455.5	3.1	-147.7	0.0	-461.5	-449.5
D_5	-458.5	8.7	-52.9	0.0	-475.5	-441.5	D_5	-486.6	3.7	-131.9	0.0	-493.8	-479.4
D_6	-356.1	10.0	-35.6	0.0	-375.7	-336.6	D_6	-416.0	5.0	-82.8	0.0	-425.9	-406.2

Notes: Monthly earnings loss of displaced workers due to firm closure, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects.

Table 4.8: **Detailed results from Figure 4.3**

without trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]	with trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	-8.9	8.0	-1.1	0.3	-24.6	6.8	D_{-6}	0.3	3.1	0.1	0.9	-5.8	6.3
D_{-5}	-17.6	8.1	-2.2	0.0	-33.4	-1.8	D_{-5}	9.4	3.0	3.2	0.0	3.6	15.2
D_{-4}	-33.5	7.6	-4.4	0.0	-48.5	-18.5	D_{-4}	8.5	2.7	3.2	0.0	3.2	13.8
D_{-3}	-50.9	7.2	-7.0	0.0	-65.1	-36.7	D_{-3}	1.8	2.1	0.9	0.4	-2.3	5.9
D_{-2}	-54.7	7.3	-7.5	0.0	-69.0	-40.4	D_{-2}	4.4	2.2	2.0	0.0	0.1	8.7
D_{-1}	-81.5	7.3	-11.1	0.0	-95.9	-67.2	D_{-1}	-9.0	2.2	-4.0	0.0	-13.3	-4.6
D_0	-94.3	7.0	-13.4	0.0	-108.1	-80.5	D_0	-14.7	1.8	-8.2	0.0	-18.2	-11.1
D_1	-122.5	7.7	-15.8	0.0	-137.7	-107.3	D_1	-31.6	2.9	-11.0	0.0	-37.2	-26.0
D_2	-143.3	7.4	-19.4	0.0	-157.8	-128.8	D_2	-30.5	2.3	-13.0	0.0	-35.1	-25.9
D_3	-162.5	7.6	-21.4	0.0	-177.4	-147.6	D_3	-48.8	2.4	-20.1	0.0	-53.6	-44.0
D_4	-191.4	8.1	-23.6	0.0	-207.2	-175.5	D_4	-89.7	3.0	-30.4	0.0	-95.5	-83.9
D_5	-223.6	8.7	-25.8	0.0	-240.7	-206.6	D_5	-142.7	3.5	-40.7	0.0	-149.6	-135.8
D_6	-233.9	10.0	-23.4	0.0	-253.5	-214.3	D_6	-159.1	4.7	-34.1	0.0	-168.2	-149.9

Notes: Monthly wage loss of displaced workers due to firm closure, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects.

Table 4.9: **Detailed results from Figure 4.4**

without trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]	with trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	19.4	5.0	3.9	0.0	9.5	29.3	D_{-6}	-0.4	2.7	-0.1	0.9	-5.7	4.9
D_{-5}	28.1	4.9	5.7	0.0	18.4	37.7	D_{-5}	6.2	2.5	2.5	0.0	1.3	11.1
D_{-4}	14.0	4.5	3.1	0.0	5.2	22.7	D_{-4}	8.7	2.1	4.1	0.0	4.6	12.8
D_{-3}	16.2	4.1	3.9	0.0	8.2	24.3	D_{-3}	5.6	1.6	3.5	0.0	2.5	8.8
D_{-2}	10.3	4.2	2.5	0.0	2.1	18.6	D_{-2}	6.1	1.7	3.7	0.0	2.9	9.3
D_{-1}	-14.9	4.3	-3.5	0.0	-23.3	-6.6	D_{-1}	-1.7	1.8	-1.0	0.3	-5.1	1.7
D_0	-19.6	4.0	-4.9	0.0	-27.5	-11.8	D_0	-13.5	1.4	-9.9	0.0	-16.2	-10.9
D_1	-605.2	4.2	-145.5	0.0	-613.4	-597.1	D_1	-630.9	1.7	-373.0	0.0	-634.2	-627.6
D_2	-475.4	4.2	-111.9	0.0	-483.7	-467.1	D_2	-485.8	1.7	-279.3	0.0	-489.2	-482.4
D_3	-439.5	4.4	-100.5	0.0	-448.1	-431.0	D_3	-448.3	1.8	-242.6	0.0	-451.9	-444.7
D_4	-475.3	4.5	-104.8	0.0	-484.2	-466.4	D_4	-514.1	2.1	-246.7	0.0	-518.2	-510.0
D_5	-394.8	4.8	-82.8	0.0	-404.2	-385.5	D_5	-446.4	2.4	-186.0	0.0	-451.1	-441.7
D_6	-353.2	5.4	-65.6	0.0	-363.7	-342.6	D_6	-426.1	3.1	-136.7	0.0	-432.3	-420.0

Notes: Monthly earnings loss of displaced workers due to collective dismissals, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects.

Table 4.10: **Detailed results from Figure 4.5**

without trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]	with trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	37.2	4.8	7.7	0.0	27.8	46.7	D_{-6}	-0.4	2.4	-0.2	0.9	-5.1	4.2
D_{-5}	32.0	4.7	6.8	0.0	22.8	41.3	D_{-5}	6.3	2.2	2.9	0.0	2.0	10.6
D_{-4}	5.7	4.3	1.3	0.2	-2.7	14.1	D_{-4}	8.4	1.8	4.6	0.0	4.8	12.0
D_{-3}	-9.6	4.0	-2.4	0.0	-17.3	-1.8	D_{-3}	5.6	1.4	4.0	0.0	2.8	8.4
D_{-2}	-11.2	4.0	-2.8	0.0	-19.1	-3.3	D_{-2}	5.2	1.5	3.6	0.0	2.4	8.1
D_{-1}	-25.4	4.1	-6.2	0.0	-33.4	-17.4	D_{-1}	-3.9	1.5	-2.5	0.0	-6.9	-0.8
D_0	-44.6	3.8	-11.7	0.0	-52.1	-37.1	D_0	-16.1	1.2	-13.4	0.0	-18.5	-13.8
D_1	-74.8	4.4	-17.2	0.0	-83.3	-66.3	D_1	-28.3	1.9	-14.8	0.0	-32.0	-24.5
D_2	-89.6	4.1	-21.7	0.0	-97.7	-81.5	D_2	-38.1	1.6	-23.6	0.0	-41.2	-34.9
D_3	-105.2	4.2	-24.9	0.0	-113.5	-97.0	D_3	-60.8	1.6	-37.3	0.0	-64.0	-57.6
D_4	-125.9	4.4	-28.3	0.0	-134.6	-117.2	D_4	-82.2	1.9	-43.3	0.0	-85.9	-78.5
D_5	-127.5	4.6	-27.9	0.0	-136.5	-118.6	D_5	-108.3	2.0	-52.9	0.0	-112.3	-104.3
D_6	-122.0	5.1	-23.8	0.0	-132.0	-111.9	D_6	-130.5	2.6	-49.8	0.0	-135.7	-125.4

Notes: Monthly wage loss of displaced workers due to collective dismissals, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects.

Table 4.11: **Detailed results from Figure 4.6**

without trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]	with trends	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	-29.3	5.7	-5.1	0.0	-40.5	-18.1	D_{-6}	3.0	3.6	0.8	0.4	-4.1	10.1
D_{-5}	2.6	5.5	0.5	0.6	-8.3	13.4	D_{-5}	3.1	3.3	1.0	0.3	-3.3	9.5
D_{-4}	4.4	4.8	0.9	0.4	-5.0	13.8	D_{-4}	4.2	2.5	1.7	0.1	-0.7	9.2
D_{-3}	35.5	4.4	8.0	0.0	26.8	44.2	D_{-3}	8.8	2.0	4.4	0.0	4.9	12.8
D_{-2}	36.8	4.5	8.2	0.0	28.0	45.6	D_{-2}	4.7	2.1	2.3	0.0	0.7	8.7
D_{-1}	23.1	4.6	5.1	0.0	14.1	32.0	D_{-1}	-2.3	2.1	-1.1	0.3	-6.5	1.9
D_0	35.6	4.3	8.3	0.0	27.2	43.9	D_0	-13.3	1.7	-7.7	0.0	-16.7	-9.9
D_1	-605.8	4.5	-134.5	0.0	-614.6	-596.9	D_1	-721.0	2.2	-331.9	0.0	-725.3	-716.8
D_2	-479.1	4.7	-102.7	0.0	-488.3	-470.0	D_2	-598.2	2.3	-261.4	0.0	-602.7	-593.7
D_3	-451.5	4.8	-94.2	0.0	-460.9	-442.1	D_3	-584.8	2.4	-242.3	0.0	-589.5	-580.1
D_4	-434.3	4.9	-88.0	0.0	-443.9	-424.6	D_4	-587.8	2.6	-224.2	0.0	-593.0	-582.7
D_5	-436.4	5.3	-83.1	0.0	-446.7	-426.1	D_5	-596.8	3.1	-195.0	0.0	-602.8	-590.8
D_6	-400.8	6.0	-66.7	0.0	-412.6	-389.0	D_6	-585.4	4.0	-147.5	0.0	-593.2	-577.7

Notes: Monthly earnings loss of displaced workers due to individual dismissals, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects.

Table 4.12: **Detailed results from Figure 4.7**

without trends							with trends						
	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]		Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	-10.3	4.6	-2.3	0.0	-19.2	-1.4	D_{-6}	1.4	2.6	0.5	0.6	-3.7	6.4
D_{-5}	-14.7	4.5	-3.3	0.0	-23.5	-5.9	D_{-5}	5.9	2.4	2.5	0.0	1.2	10.5
D_{-4}	-23.5	4.1	-5.8	0.0	-31.4	-15.6	D_{-4}	6.0	2.0	3.1	0.0	2.2	9.9
D_{-3}	-13.8	3.8	-3.6	0.0	-21.2	-6.4	D_{-3}	7.1	1.6	4.5	0.0	4.0	10.1
D_{-2}	-13.4	3.8	-3.5	0.0	-20.9	-6.0	D_{-2}	3.9	1.6	2.5	0.0	0.8	7.0
D_{-1}	-27.8	3.9	-7.2	0.0	-35.4	-20.2	D_{-1}	-5.6	1.6	-3.4	0.0	-8.8	-2.4
D_0	-30.3	3.7	-8.3	0.0	-37.4	-23.1	D_0	-15.3	1.3	-11.5	0.0	-17.9	-12.7
D_1	-21.8	4.1	-5.3	0.0	-29.9	-13.8	D_1	13.8	2.0	6.8	0.0	9.8	17.7
D_2	-46.6	3.9	-11.9	0.0	-54.3	-38.9	D_2	-12.0	1.7	-7.0	0.0	-15.4	-8.6
D_3	-74.8	4.0	-18.6	0.0	-82.7	-66.9	D_3	-51.4	1.8	-28.7	0.0	-54.9	-47.9
D_4	-93.4	4.2	-22.2	0.0	-101.6	-85.1	D_4	-93.6	2.0	-46.5	0.0	-97.6	-89.7
D_5	-105.8	4.5	-23.6	0.0	-114.6	-97.1	D_5	-119.2	2.4	-50.6	0.0	-123.9	-114.6
D_6	-82.6	5.2	-15.9	0.0	-92.7	-72.4	D_6	-112.5	3.1	-35.9	0.0	-118.6	-106.4

Notes: Monthly wage loss of displaced workers due to individual dismissals, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects.

Table 4.13: **Detailed results from Figure 4.10**

without trends figure 4.4							Without trends with new comparison group						
	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]		Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	19.4	5.0	3.9	0.0	9.5	29.3	D_{-6}	15.9	5.0	3.2	0.0	6.1	25.8
D_{-5}	28.1	4.9	5.7	0.0	18.4	37.7	D_{-5}	25.5	4.9	5.2	0.0	15.9	35.2
D_{-4}	14.0	4.5	3.1	0.0	5.2	22.7	D_{-4}	11.9	4.5	2.7	0.0	3.1	20.6
D_{-3}	16.2	4.1	3.9	0.0	8.2	24.3	D_{-3}	12.8	4.1	3.1	0.0	4.7	20.9
D_{-2}	10.3	4.2	2.5	0.0	2.1	18.6	D_{-2}	6.4	4.2	1.5	0.1	-1.8	14.7
D_{-1}	-14.9	4.3	-3.5	0.0	-23.3	-6.6	D_{-1}	-17.1	4.3	-4.0	0.0	-25.5	-8.8
D_0	-19.6	4.0	-4.9	0.0	-27.5	-11.8	D_0	-22.9	4.0	-5.7	0.0	-30.7	-15.1
D_1	-605.2	4.2	-145.5	0.0	-613.4	-597.1	D_1	-605.8	4.1	-146.1	0.0	-613.9	-597.7
D_2	-475.4	4.2	-111.9	0.0	-483.7	-467.1	D_2	-476.1	4.2	-112.4	0.0	-484.4	-467.8
D_3	-439.5	4.4	-100.5	0.0	-448.1	-431.0	D_3	-445.8	4.4	-101.8	0.0	-454.4	-437.3
D_4	-475.3	4.5	-104.8	0.0	-484.2	-466.4	D_4	-483.6	4.6	-106.1	0.0	-492.5	-474.6
D_5	-394.8	4.8	-82.8	0.0	-404.2	-385.5	D_5	-401.1	4.8	-83.6	0.0	-410.5	-391.7
D_6	-353.2	5.4	-65.6	0.0	-363.7	-342.6	D_6	-361.2	5.4	-66.7	0.0	-371.9	-350.6

Notes: Monthly earnings loss of displaced workers due to firm closure, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects. Results from the left side of the table are a replication of "without trends figure 4.4". Right side of the table uses the new comparison group.

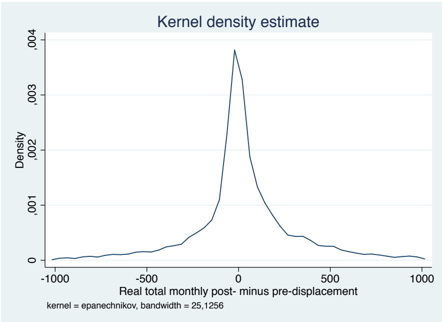
Table 4.14: **Detailed results from Figure 4.11**

without trends figure 4.4							Without trends with new comparison group						
	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]		Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
D_{-6}	-29.3	5.7	-5.1	0.0	-40.5	-18.1	D_{-6}	-29.4	5.7	-5.1	0.0	-40.6	-18.1
D_{-5}	2.6	5.5	0.5	0.6	-8.3	13.4	D_{-5}	2.5	5.5	0.5	0.6	-8.3	13.3
D_{-4}	4.4	4.8	0.9	0.4	-5.0	13.8	D_{-4}	4.4	4.8	0.9	0.4	-5.1	13.8
D_{-3}	35.5	4.4	8.0	0.0	26.8	44.2	D_{-3}	35.4	4.4	8.0	0.0	26.7	44.1
D_{-2}	36.8	4.5	8.2	0.0	28.0	45.6	D_{-2}	36.8	4.5	8.2	0.0	28.0	45.6
D_{-1}	23.1	4.6	5.1	0.0	14.1	32.0	D_{-1}	23.1	4.6	5.1	0.0	14.1	32.0
D_0	35.6	4.3	8.3	0.0	27.2	43.9	D_0	35.6	4.3	8.3	0.0	27.2	44.0
D_1	-605.8	4.5	-134.5	0.0	-614.6	-596.9	D_1	-605.7	4.5	-134.5	0.0	-614.6	-596.9
D_2	-479.1	4.7	-102.7	0.0	-488.3	-470.0	D_2	-479.1	4.7	-102.6	0.0	-488.2	-469.9
D_3	-451.5	4.8	-94.2	0.0	-460.9	-442.1	D_3	-451.5	4.8	-94.2	0.0	-460.9	-442.1
D_4	-434.3	4.9	-88.0	0.0	-443.9	-424.6	D_4	-434.2	4.9	-88.0	0.0	-443.9	-424.6
D_5	-436.4	5.3	-83.1	0.0	-446.7	-426.1	D_5	-436.4	5.3	-83.1	0.0	-446.7	-426.1
D_6	-400.8	6.0	-66.7	0.0	-412.6	-389.0	D_6	-400.8	6.0	-66.7	0.0	-412.6	-389.0

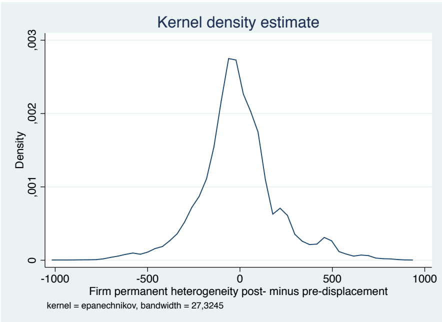
Notes: Monthly earnings loss of displaced workers due to firm closure, including transitions to zeros (2008 Euros). In the regressions we control for age and age squared and calendar year fixed effects. Results from the left side of the table are a replication of "without trends figure 4.6". Right side of the table uses the new comparison group.

Appendix D

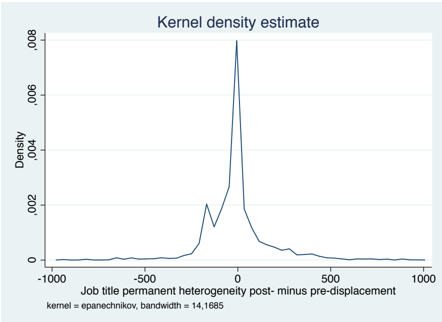
Figure 4.12: **The empirical distribution of wages of displaced workers: post- minus pre-displacement (based on Figure 4.9)**



(a) Monthly wage



(b) Firm permanent heterogeneity



(c) Job title permanent heterogeneity

Notes: Displaced workers' density distributions difference between one year after displacement and one year before displacement.

Appendix E - The three-way high-dimensional fixed effects regression model

As discussed above, the main contribution of this study is to decompose the earnings losses due to job displacement into its main sources. To do so the JLS methodology is extended by incorporating firm and job title fixed effects in the wage equation defined in (1) and (2). This extension is made for the monthly wage and discards observations where labor earnings are zero.

In fact, the QP data set provides a rich set of information that enables us to identify firms and job titles. Each firm entering the database is assigned a unique identification number, which allows tracking them over the years. Furthermore, for each worker we are able to identify the occupational category in each collective agreement.

It is worth noting that the Ministry of Employment collects the QP data in order to check if employers are complying with the wage floors agreed upon for each occupational category. The collective agreement defines wage floors for each job title (called *categoria profissional*). On average, the collective agreement defines the wage floor for around 100 job titles. Overall, in a given year, one can classify each worker according to about 30,000 job title collective agreement combinations.[22]

Thus, we are confident that by incorporating job title fixed effects in the wage regression we can account well for job title heterogeneity, and by so doing, we should be able to provide refined estimates (filtered from job title heterogeneity) of worker and firm fixed effects.

The baseline specification is:

[22]It should be noticed that workers in the same occupational category may have different wages, as they are covered by a different collective agreement, e.g., a secretary in the banking industry agreement, as opposed to a secretary in the retail trade collective agreement.

$$w_{ijft} = \alpha_i + \theta_f + \lambda_j + \gamma_t + \beta X_{it} + \epsilon_{ijft} \tag{5}$$

where α_i is a worker fixed effect, θ_f is a firm fixed effect and λ_j is a job title fixed effect. w_{ijft} represents the monthly wage for each individual i in job j working for firm f in year t. X_{it} controls for age and age squared for each individual i in year t, γ_t are calendar year fixed effects, ϵ_{ijft} is assumed to follow the conventional assumptions.

In order to estimate this model that incorporates three high-dimensional fixed effects we need to use a modified version of the methodology initially developed by Abowd et al. (1999) and Abowd et al. (2002) (developed by Guimarães and Portugal (2010) to work with large datasets and for three fixed effects).

In matrix format, the stacked system has the following form:

$$\mathbf{W} = \alpha \mathbf{F_1} + \theta \mathbf{F_2} + \lambda \mathbf{F_3} + \phi \mathbf{Z} + \epsilon \tag{6}$$

In this equation, F_1, F_2, and F_3 are high-dimensional matrices for the worker, firm and job fixed effects, respectively. Z is a matrix of the explanatory variables and calendar year fixed effects from equation (5).

The least squares estimator of ϕ, α, θ, and λ solve the following equations:

$$\begin{bmatrix} Z'Z & Z'F_1 & Z'F_2 & Z'F_3 \\ F_1'Z & F_1'F_1 & F_1'F_2 & F_1'F_3 \\ F_2'Z & F_2'F_1 & F_2'F_2 & F_2'F_3 \\ F_3'Z & F_3'F_1 & F_3'F_2 & F_3'F_3 \end{bmatrix} \begin{bmatrix} \phi \\ \alpha \\ \theta \\ \lambda \end{bmatrix} = \begin{bmatrix} Z'W \\ F_1'W \\ F_2'W \\ F_3'W \end{bmatrix} \tag{7}$$

It is computationally difficult to invert the left matrix due to the large number of workers, firms, and job titles. Herein we use an iterative solution that alternates between estimation of ϕ, α, θ, and λ.

$$\begin{bmatrix} \phi \\ \alpha \\ \theta \\ \lambda \end{bmatrix} = \begin{bmatrix} (Z'Z)^{-1}Z'(W - \alpha F_1 - \theta F_2 - \lambda F_3) \\ (F_1'F_1)^{-1}F_1'(W - \theta F_2 - \lambda F_3 - \phi Z) \\ (F_2'F_2)^{-1}F_2'(W - \alpha F_1 - \lambda F_3 - \phi Z) \\ (F_3'F_3)^{-1}F_3'(W - \alpha F_1 - \theta F_2 - \phi Z) \end{bmatrix}$$

It is clear from the previous equations that at each iteration the fixed effects are simply computed as averages of the residuals. For an example, $(F_3'F_3)^{-1}F_3'$ is simply a demeaning operator for the job title fixed effect. The iteration protocol was developed by Guimarães and Portugal (2010). The iterative solution alternates between estimation of ϕ, α, θ, and λ and proceeds as follows. First, the algorithm makes use of the Frish-Waugh-Lovell theorem to remove the influence of the three high-dimensional fixed effects from each individual variable. Through the recursive algorithm the current value of ϕ can be used to estimate the current value of α. In estimating θ the previous values of ϕ and α are used. In estimating λ the previous values of θ, ϕ, and α are used. Then the algorithm restarts and will converge because the parameter updates are chosen according to the equations in (7). Next, we estimate the regression using the transformed variables with a correction to the degrees of freedom. This approach yields the exact least squares solution for the coefficients and standard errors.

The fixed effects in equation (5) were estimated using the complete data set that covers the employed population in the private sector in Portugal with all available information from 1986 to 2008. The identification of the three high dimensional fixed effects given by the worker, firm and job title effects was circumvented by applying the algorithm by Abowd et al. (2002) , based on graph theory to determine groups of connected individuals, firms and job titles. A connected group exists when at least one element of a worker, job title and firm links the rest of the group. The largest connected group represents more than 99% of the sample.[23]

[23]The worker and the job title fixed effect were normalized to have average zero while the firm fixed effect was not normalized.

Appendix F - Decomposition of the wage loss including tenure - displaced workers due to firm closure

Table 4.15: **Decomposition of the wage loss - displaced workers due to firm closure (replication of table 4.3 including tenure)**

Period relative to displacement	Base OLS hourly wage	Full OLS hourly wage	$\delta_k^{base} - \delta_k^{full}$	Worker fixed effect	Firm fixed effect	Occupation fixed effect	Tenure
D_{-6}	-270.2	17.3	-287.6	-135.6	-214.7	62.2	0.5
D_{-5}	-278.2	8.7	-286.9	-130.4	-214.8	57.9	0.3
D_{-4}	-295.4	-2.5	-292.8	-152.8	-181.9	41.7	0.2
D_{-3}	-298.7	-18.7	-280.0	-141.9	-184.8	46.2	0.5
D_{-2}	-322.0	11.0	-333.0	-151.0	-215.4	33.1	0.3
D_{-1}	-395.5	-10.9	-384.6	-168.0	-221.0	3.9	0.5
D_0	-376.1	32.0	-408.1	-172.8	-229.4	-6.1	0.3
D_1	-421.2	-12.7	-408.5	-137.4	-238.1	-33.1	0.1
D_2	-492.6	-7.0	-485.6	-178.8	-253.3	-54.6	1.1
D_3	-514.6	1.9	-516.5	-185.7	-265.0	-67.8	2.1
D_4	-574.7	-12.8	-561.9	-198.8	-300.6	-65.4	2.9
D_5	-508.0	15.8	-523.8	-180.5	-290.6	-56.4	3.8
D_6	-492.3	-39.8	-452.5	-119.3	-302.4	-35.4	4.7
$D_{-6} - D_0$	-319.4	5.3	-324.7	-150.4	-208.9	34.1	0.4
$D_1 - D_6$	-500.6	-9.1	-491.4	-166.8	-275.0	-52.1	2.5
Δ	-181.1	-14.4	-166.7	-16.4	-66.2	-86.3	2.1
Results in percentage							
$D_{-6} - D_0$	-39.0	0.6	-39.6	-18.4	-25.5	4.2	0.0
$D_1 - D_6$	-61.1	-1.1	-60.0	-20.4	-33.6	-6.4	0.3
Δ	-22.1	-1.8	-20.4	-2.0	-8.1	-10.5	0.3

Notes: This table reports the Gelbach decomposition of the three fixed effects of the wage loss of displaced workers. In the regressions we control for age and age squared and calendar year fixed effects. In each column, $D_{-6} - D_0$ is the computed average between the first seven lines (D_{-6} to D_0). $D_1 - D_6$ is the computed average between the next six lines (D_1 to D_6). In the line Δ we compute the difference between the previous two lines. In the last three lines we compute the results in percentage by dividing the respective numbers by the average wage of displaced workers in the pre displacement period (819 euros). Tenure is computed as the sum between tenure and tenure squared multiplied by the respective coefficients from the full model.

Table 4.16: **Decomposition of the wage loss - displaced workers due to firm closure (replication of table 4.3 including tenure)**

Period relative to displacement	Base OLS hourly wage	Full OLS hourly wage	Tenure
D_{-6}	-270.2	-269.0	-1.2
D_{-5}	-278.2	-277.8	-0.4
D_{-4}	-295.4	-293.7	-1.6
D_{-3}	-298.7	-298.0	-0.6
D_{-2}	-322.0	-320.1	-2.0
D_{-1}	-395.5	-393.9	-1.6
D_0	-376.1	-372.0	-4.0
D_1	-421.2	-362.7	-58.5
D_2	-492.6	-440.5	-52.1
D_3	-514.6	-469.3	-45.3
D_4	-574.7	-534.6	-40.0
D_5	-508.0	-472.3	-35.7
D_6	-492.3	-462.1	-30.1
$D_{-6} - D_0$	-319.4	-317.8	-1.6
$D_1 - D_6$	-500.6	-456.9	-43.6
Δ	-181.1	-139.1	-42.0
Results in percentage			
$D_{-6} - D_0$	-39.0	-38.8	-0.2
$D_1 - D_6$	-61.1	-55.8	-5.3
Δ	-22.1	-17.0	-5.1

Notes: This table reports the Gelbach decomposition of the wage loss of displaced workers. In the regressions we control for age and age squared and calendar year fixed effects. In each column, $D_{-6} - D_0$ is the computed average between the first seven lines (D_{-6} to D_0). $D_1 - D_6$ is the computed average between the next six lines (D_1 to D_6). In the line Δ we compute the difference between the previous two lines. In the last three lines we compute the results in percentage by dividing the respective numbers by the average wage of displaced workers in the pre displacement period (819 euros). Tenure is computed as the sum between tenure and tenure squared multiplied by the respective coefficients from the full model.

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